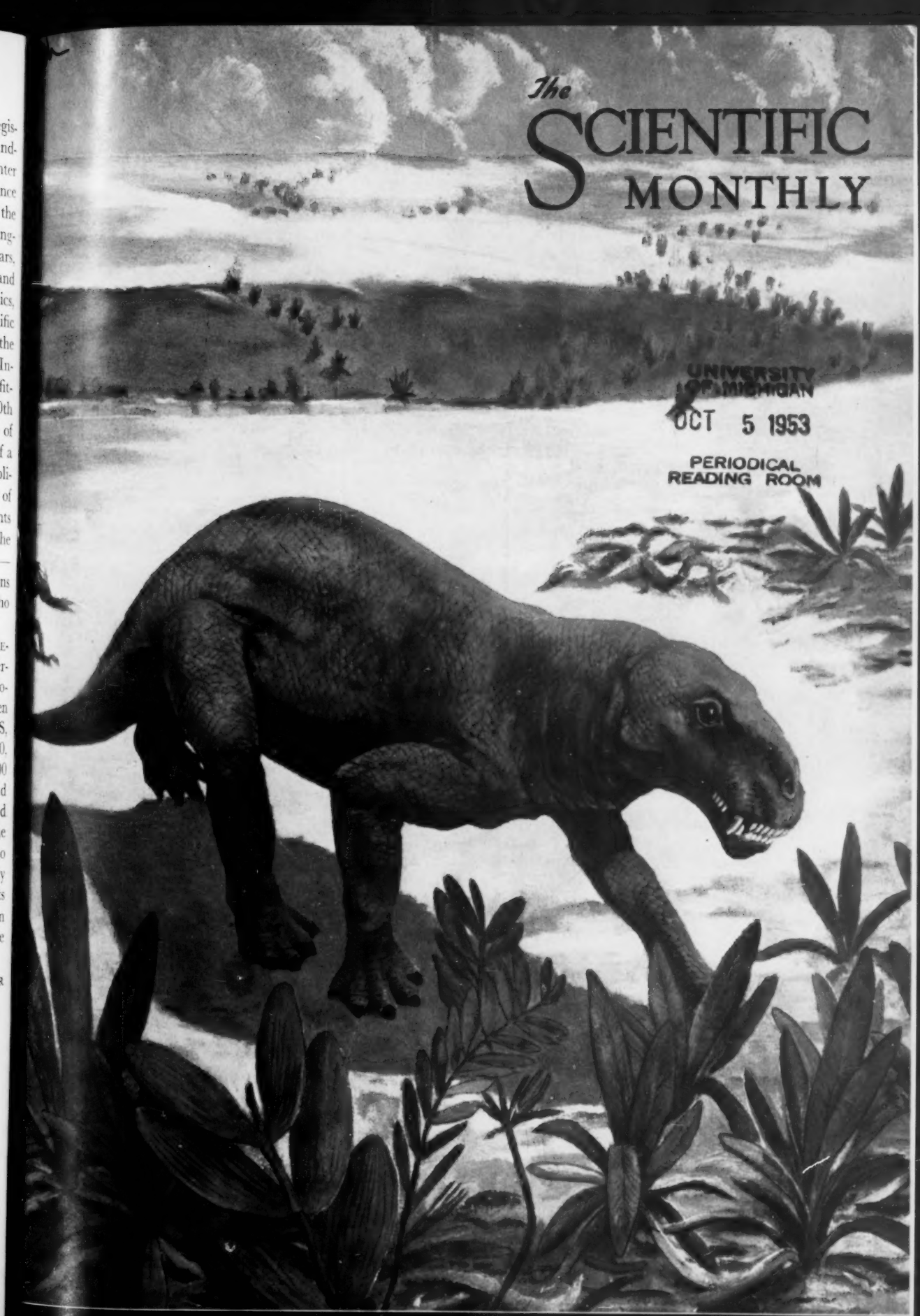

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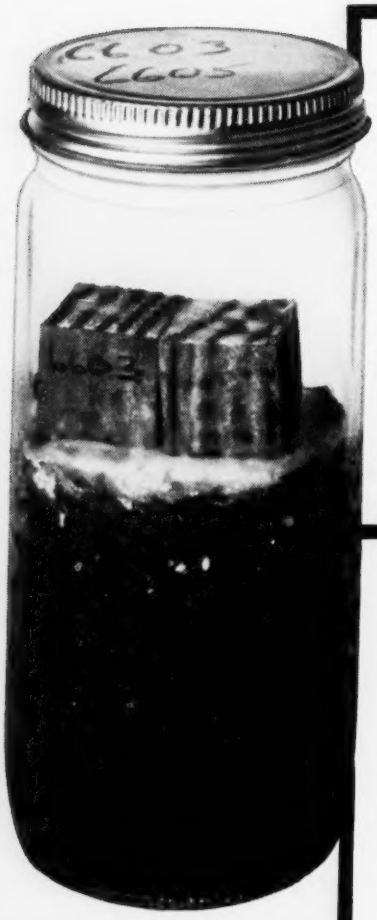

THIS BOTTLE TURNS SEVEN YEARS INTO SEVEN MONTHS

Test blocks of pole wood are fed to destructive fungi in bottles like this at Bell Laboratories. Wood rests on soil which controls moisture conditions and promotes fungus growth. Test speeds search for better preservatives.

This year the Bell System is putting 800,000 new telephone poles into service. How effectively are they preserved against fungus attack and decay?

Once the only way to check a preservative was to plant treated wood specimens outdoors, then wait and see—for seven years at least. Now, with a new test devised in Bell Laboratories most of the answer can be obtained in seven months.

Cubes of wood are treated with preservatives, then enclosed in bottles with fungus of the most destructive kind, under temperature and humidity conditions that accelerate fungus activity. Success—or failure—of fungus attack on cubes soon reveals the best ways to preserve poles.

The test has helped show how poles can be economically preserved for many years. It is another example of how Bell Telephone Laboratories works to keep down your telephone costs.

A boring is taken from a pole section to see how far preservative has penetrated. For poles to last, it must penetrate deeply and be retained for a long time.

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THE SCIENTIFIC MONTHLY

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New Books Received

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THE SCIENTIFIC MONTHLY

SEPTEMBER 1953

The Diary of William Fellowes Morgan

TEMPLE R. HOLLCROFT

Dr. Hollcroft has been Professor of Mathematics and Department Chairman at Wells College for thirty-four years, and was recently appointed Wells College Historian. His five degrees were conferred by Hanover College (B.S., A.B., and Sc.D.), University of Kentucky (A.M.), and Cornell University (Ph.D.). In World War I he was a 2nd Lieut. in the Field Artillery. He spent two years studying and traveling in Europe. He is a member of four mathematical societies, four honorary societies, including Phi Beta Kappa and Sigma Xi, and fellow of the AAAS. Dr. Hollcroft was Associate Secretary of the American Mathematical Society for fourteen years. He has published 38 research papers in mathematics, and many articles on local history.

WILLIAM FELLOWES MORGAN kept a diary of a portion of an expedition to Colorado and New Mexico with his great-uncle, Lewis Henry Morgan, in the summer of 1878. The chief purpose of this expedition was to study the prehistoric ruins in that region and the buildings and customs of the Pueblo Indians. The diary also contains an account of the 27th meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE in St. Louis, which they attended.

Mrs. Cleveland E. Dodge of New York City, a daughter of William Fellowes Morgan and Vice Chairman of the Wells College Board of Trustees, presented this diary to the Wells College Library on March 21, 1953.

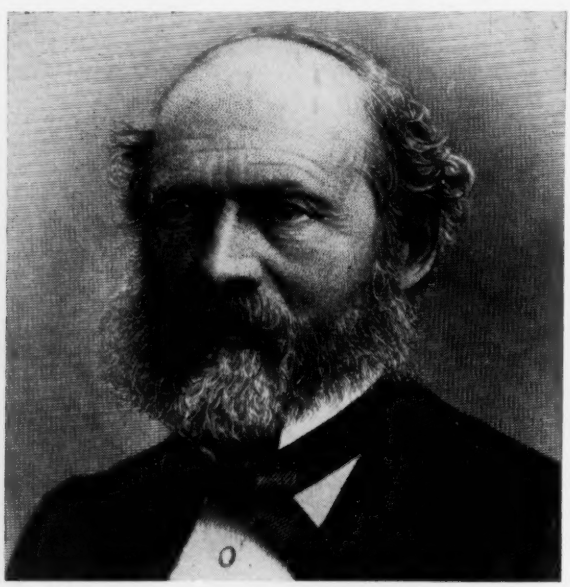
A short note under the caption "Has he a rival?" was published in SCIENCE (**117**, 234 [1953]). In this note it was stated that William Fellowes Morgan, who became a member of the AAAS at the age of 17, may have been the youngest member ever elected. Now it is revealed in the diary that he presented a paper to the AAAS on the day following his election to membership and that his paper received high commendation from the scientists who heard it. This was a remarkable accomplishment for a youth of 17, who had just completed his second year at college.

The emigrant ancestor of both Lewis Henry Morgan (1818–1881) and William Fellowes Morgan (1860–1943) was James Morgan (1607–1685), a native of Wales, who came to Boston, Massachusetts, in 1636. One of his sixth-generation descendants, Jedediah Morgan (1774–1826), moved in 1792 with his parents to a farm near Aurora, New York, and later became a New York state senator. Two of Jedediah Morgan's sons were Lewis Henry Morgan, "the father of American anthropology" and President of the AAAS in 1880, and Amos Morgan, the father of David Pierce Morgan, an eminent banker of New York

City, who married Catherine Fellowes, daughter of William Fellowes. Their oldest son, William Fellowes Morgan, the author of the diary, was born September 24, 1860.

Lewis Henry Morgan was born November 21, 1818, on his father's farm (where his grandfather had settled in 1792), three miles south of Aurora, New York. His father moved to Aurora when Lewis was four years old. Lewis attended Cayuga Academy in Aurora until 1838 when he entered Union College as a junior, graduating in 1840. Christened "Lewis Morgan," he added "Henry" to his name at about the time he started to college. In 1840 he returned to Aurora and studied the classics in Cayuga Academy and law in the office of David Wright. It was about this time that he organized "The Grand Order of the Iroquois" among the students of Cayuga Academy. Beginning at Aurora, this society soon established "tribes" in several other localities in central New York. The G.O.I. was both an expression of Morgan's growing interest in Indian life and customs and an incentive to the development of that interest. Many of his researches were begun in order to model the G.O.I. after the structure of the Iroquois social and political systems. His first book, "The League of the Iroquois," published in 1851, consists largely of a collection of addresses written for the G.O.I. and delivered at its meetings.

Lewis Henry Morgan moved to Rochester in 1845 and soon began writing the articles and books, chiefly on anthropology, that made him famous. His business ventures were more successful than his law practice, and he became moderately wealthy. He served in both the Assembly and the Senate of the New York Legislature during the sixties. Interested in the education of women, he bequeathed his estate to the University of Rochester to be used in establishing a college for women at that university, of which he was a trustee. He was also the first elected trustee of Wells College. He organized the Anthropology Subsection D of Section B of the AAAS, was its chairman for some years, and was President of the AAAS in 1880. He presided at the 1880 Boston meeting, but was too ill to deliver an address as retiring president at the 1881 Cincinnati meeting. His health failed rapidly, but he was able to complete his last book, *Houses and House Life of the American Aborigines* (Department of the Interior, Washington, 1881; referred to hereafter as *House Life*). A biography of Morgan, written by F. W. Putnam, Permanent Secretary of the AAAS for 25 years and President in 1898 (*Proceedings of the American Association for Arts and Sciences,* Vol. 117 [1882]) contains the concluding sentence: "The great principles which his researches have brought out . . . will ever stand . . . as foundations upon which to build in the further study of America in archeology and ethnology."



Lewis H. Morgan, anthropologist, the uncle of W. F. Morgan. (Dept. of History, Bureau of American Ethnology.)

William Fellowes Morgan graduated at Columbia University in 1880 and received the additional degree of Mining Engineer there in 1884. He married Emma Leavitt in 1885. As a scientist, he was among the first to foresee the possibilities of preservation by refrigeration. He entered the cold storage business in 1887 and was soon made president of the company. He was a director in several other companies and, in 1905 and 1907, a member of the New Jersey Legislature. He held offices in many educational, religious, and philanthropic institutions, including President of the Y.M.C.A. Association of New York City, alumni trustee of Columbia University for six years, trustee of Wells College for 26 years, for 13 of which he was President of its Board of Trustees.

Although his business activities, which he pursued with marked success, diverted him from a life of science, William Fellowes Morgan was always interested in that field and continued his membership in scientific organizations, including the AAAS and the New York Academy of Sciences. When he died in 1943, he had been a member of the AAAS for 65 years. He and five others were eulogized in the article, "Six Patriarchs of the Association," published in the January 1945, *AAAS Bulletin*. He was elected a Fellow in 1932 and an Emeritus Life Member in 1933. These honors, together with

William Fellows Morgan, about the time he wrote the diary.

the fact that he became a member and presented a paper at the age of 17, constitute a unique record in the annals of the AAAS.

Lewis Henry Morgan, with two of his grandnephews, William Fellowes Morgan and his younger brother, David Percy Morgan, went on the expedition to the Southwest in the summer of 1878. They were accompanied by two other young men called "Donald" and "Henry" in the diary. They visited the Pueblo Indians in their villages and made a survey of many ruins of prehistoric dwellings in southwestern Colorado and northern New Mexico.

The party first went by train to Cañon City, at that time the western terminus of the Denver & Rio Grande Railroad. For about six weeks thereafter, they traveled on ponies and in wagons several hundred miles through an extremely rough and largely unsettled country. Lewis Henry Morgan published the findings of this expedition in *House Life*. On page 172, he states that they visited pueblo ruins near the Animas River on July 22, 1878 (one of the very few dates given). Probably a few days previously, they had studied a newly discovered cliff-house on the Mancos River. This cliff-house was the one described by William Fellowes Morgan in his AAAS paper. After viewing other ruins of prehistoric dwellings, including some on the San Juan River, the party arrived at Conejos, Colorado, on August 5.

The diary of William Fellowes Morgan begins with their arrival at Conejos. Here they obtained a team and a "Kansas Wagon" in which they traveled to Taos Pueblo and back, a distance of about 175 miles, and then from Conejos to Alamosa. At that time, Alamosa was the southern terminus of the Denver & Rio Grande Railroad. They continued the journey by rail via Denver to St. Louis to attend the AAAS meeting. The following portion of the diary ends with their departure from St. Louis.

Costilla [New Mexico], August 6, 1878.

When we arrived at Guadalupe, or Conejos[1] as it is usually called, we stabled our horses in a barn belonging to J. M. Archeluta, a Spanish merchant, and took our supper at a hotel kept by Joe Coleman. After supper, as we felt too lazy to put up the tent, we agreed to spread our blankets in the hay in the barn. We spent a pretty comfortable night and were waked early by the chickens, for we were surrounded by them. After breakfast, we busied ourselves trying to get a team to take us to Taos. We at last made arrangements to go to Taos and leave Henry. We made some purchases and saw Henry off. We had engaged a team from Joe Coleman, the landlord. It consisted of a Kansas Wagon and two sorrel horses and a Mexican driver.

We started about 9½ o'clock. We drove 18 miles to the Rio Grande, forded at Mayer's Crossing and stopped for lunch. Our drive during the morning had been over a lava field and was very level, being through the San Luis Valley. It was a very pretty ride. On all sides we had magnificent mountain views of the Sangre de Cristo range and Mt. Blanca, also of the Costilla, Conejos, and other smaller ranges.

The Rio Grande at this time was quite low, so that few people availed themselves of the ferry at this point. After stopping a few minutes at the river to feed the horses and ourselves, we drove on to Costilla, 12 miles further. Costilla is a moderately large plaza, 30 miles nearly east of Conejos. To get to it you have to cross from one side of the San Luis Valley to the other. It is situated at the base of the Costilla range. We camped near the hotel kept by Mr. George Blackmore. Uncle Lewis,[2] Donald and I went to the hotel. We prepared for tea by taking a swim in a small bathing pool near the hotel. We had a very good supper and then went to our tent. We were much amused by some Mexican boys who came to our tent and seemed much surprised at our mode of living.

Taos [N. M.], August 9, 1878.

We got up early Wednesday, August 7. After breakfasting at the hotel, we left about 6:45 and drove down the San Luis Valley over a level road for 20 miles or so. We camped for lunch on the

North Town, Taos, N. M., from the southwest. (Courtesy Bureau of American Ethnology.)

Rio Colorao, or Red River. We took lunch with some drummers[3] from St. Louis, one named Howison. After lunch we had a pretty hard pull, our road leading over quite a number of steep hills before coming to the plaza of San Cristobal. We went past this plaza up onto the mesa and had a level road for some distance. After driving about two miles to Arroyo Hondo, we camped in the street in the middle of the Plaza.

We got up early and started for Taos before taking breakfast, as we were only 12 miles from it. We started at 5:20, our road lay over several mesas so that we were obliged to walk up the hills from one mesa to another. We arrived at Taos at about 8 A.M. Taos is a very old Mexican town and has been the home of Kit Carson, Gov. Bent[4] and other prominent men. The main business portion is situated on a large square. The houses are all one story and have but few windows except on the inner or court side. This custom is a remnant of old times when the Indians attacked the towns, but now when there is no danger the old custom still remains. We drove through several narrow but pretty clean streets, for Taos has its streets swept regularly. We came to the main square and drove to the hotel kept by H. Dibble. Here we got a good breakfast together with the St. Louis drummers who had come down from Costilla at the same time we had. After breakfast, Mr. Dibble gave me an Apache jug and at the same time I bought from him a handsome San Juan vase. We then engaged a Mr. Scherrick as interpreter and got Mr. Müller to go with us and introduce us to the pueblo Indians. We started for the pueblo village 3 miles from town. This pueblo village, consisting of two large pueblo buildings, one on each side of the river called "casa grande," and numerous smaller buildings, is situated on the Taos River 3 miles from the Mexican town of Fernandez de Taos. We drove up to the pueblo on the south side of the river to the dwelling of the "cacique" or head of the tribe. This man, of the Concha family, is blind and is always led around by his son who is the head war chief of the tribe. We stopped by his house and went up the ladder to the roof of the house. We were then introduced to Jose Maria, the "alcalde" or governor. This man is a very smart Indian and has a good face. After waiting a little while, we were introduced to the cacique who had come up. We had laid in a supply of tobacco at Taos which now became useful. We were taken to an open place above the pueblos on the north side of the river. Uncle Lewis now busied himself learning all he could about the customs, etc. of the Indians.[5]

In the meanwhile, I got out a photograph of the pueblo which amused the Indians considerably. We next put up our tent and got lunch, after which we began to buy things from the Indians. This kept us busy all the afternoon and evening. In the

evening we were much interested listening to the Indians singing their dance songs. It is great fun sitting in the tent surrounded by Indians, giving them pipes to smoke and crackers to eat.

After getting rid of them all, which the alcalde helped us to do, we put our purchases in order and went to bed. We got up early the next morning, Friday, August 9, and began our trading again, for the Indians were ready before we got up. After a breakfast of milk and crackers, we went out to visit some of the rooms in the pueblo. After I got back to the tent, the cacique and all of the chiefs of the tribe came in from their work in the fields. They sat down in the tent and we boys entertained them with cigarettes, etc. and with crackers spread with apple butter. As Uncle Lewis did not come back from visiting the pueblo, the chiefs went to the room of the alcalde and waited for him there. I went also and smoked a "cherook" with them. We had quite a pow-wow and then took leave of them all. As our small change had given out, we arranged with the Indians to come to town and get their pay. We then packed up our purchases and started off. On our way to town we picked up Juanita, a nice clean squaw, and Juan Jose Cuasso, an Indian to whom we owed some money.

We arrived in town and settled with the Indians. We then entrusted our purchases to Mr. Burckert to have them packed and sent to New York. After dinner at the hotel we started for home. Our drive was over the same ground we had been over coming down. We went past our old camp at Arroyo Hondo and went on to San Cristobal where we camped in a pouring rain. Our visit to the Indians had been very satisfactory. We had got an insight into their way of living. For the last three years their crops have been badly damaged by grasshoppers which had made them so poor that they were willing to sell their pottery, etc. for anything. We treated them well and so they were very kind and accommodating to us.

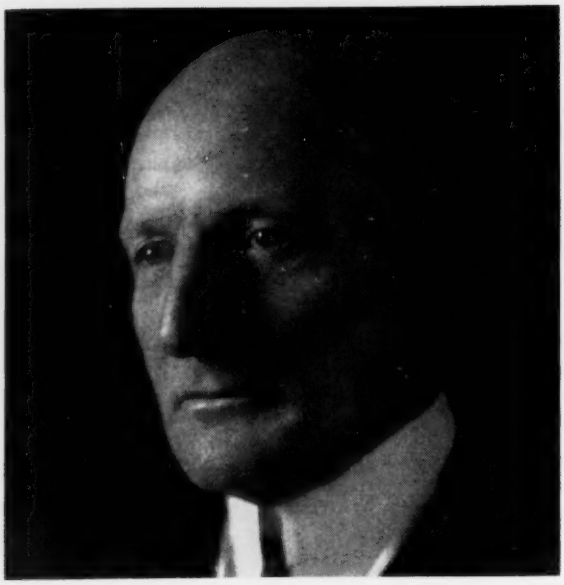
Conejos [Colorado], August 11, 1878.

We left our camp on the San Cristobal early Saturday, August 10. Soon after we had started it began to rain and it continued nearly all day. We boys walked most of the day so as not to catch cold. We had pretty heavy pulling up steep hills in the mud, for the adobe of this country makes a more sticky mud than any I have ever seen. It was all we could do to move. We camped this side of Rio Colorao and procured some "tortillas." These are merely unleavened cakes and are as tough as leather. After lunch we drove on through mud and gravel until at last we came back to Costilla. Here we stopped for the night with Mr. Blackmore. We left Costilla about 8 o'clock Sunday morning, August 11. We had a bright sunny day which made a nice contrast with the day before. Just before we came to the Rio Grande we saw an antelope. After crossing the river, we camped. We boys amused ourselves washing our clothes and wading across to a little island in the river. On this island I found a very pretty piece of rock which Uncle Lewis told me was "obsidian." After staying some time at the river, we went over the lava field to Conejos. We arrived here about 5 o'clock. We spread our beds in a large room of the hotel. We did not have a good supper. In the evening Uncle Lewis had a long conservation with Major Head,[6] ex-Lieut. Gov. of Colorado, who lives here.

Cañon City [Colorado], August 13, 1878.

We left Conejos the next morning, Monday, August 12, with the same team. We now drove northward to Alamosa, gradually approaching the mountains. We drove about 12 miles when we met a large train of bullwhackers.[7] Next we saw a coyote. We had already seen two of them two days before. This one did not seem at all frightened at our approach. Donald fired a shot at him and he ran off. After driving a little further we camped for lunch by Alamosa Creek. We then walked on ahead of the wagon and met Gen. Heffernan[8] going back home to Animas City. We got on very well until we came to a very sandy part of the road where we had a hard pull. We got into Alamosa about 4 P.M. We first found Mr. Simpson, the baggage agent. We next got our traps which Henry had left. These we packed and directed for home. We then went to the Perry House and engaged rooms for the night. In the evening we met Judge Ira B. Felton who told us how he had met Will Sloan and had loaned him some money. Alamosa is the terminus of the D.&R.G.R.R. It is a mushroom town as it was begun only a month or so ago and now is well advanced, many stores being completed and many others nearly so. It is the old town of Garland City[9] taken to pieces and put up here. The sound of the hammer can be heard from morning till night. Everyone is busy getting ready for winter.

We were called at 5 A.M. and went to breakfast. Our appearance was very ludicrous, our hats were large freighters' hats, very dirty, our pants were overhauls very much spotted with axle grease, ink, etc. My shoes were all worn out, so much so that when I asked the Porter after he had blacked them how much he would charge me, he said, "The



William Fellows Morgan in later life.

general price is 25 cts., but I will let you have it for 10 cts." We got our seats on the train which starts at 6:20. I now met a friend named Joy from Columbia School of Mines. He is out here in the lumber business at Placer. The fireman on this train is from N. Y. He asked us to tell his father, Charles Freeman, who lives near the McCombs Dam Bridge, that he is well. After the train had been out a little while I saw 4 antelopes a moderate distance from the railroad. We passed Fort Garland and then the remains of Garland City.[9] After going a few more hours we came to the summit, 9339 ft. high. The descent of the Veta Pass was very beautiful. We had just left Blanca and Baldy peaks in all their glory and now we came into full view of the Spanish Peaks, or as the Indians call them, [Huajatolla],[10] which means "Twin Breasts" and it is a good name for they have that appearance very distinctly. After going down some distance we came to Dump Mountain. The railroad goes nearly around it, so some people get out and walk across and meet the train in the Mule-Shoe Curve. As we go around the end of Dump Mountain, we get a very pretty view of the valley. After getting down from the mountains, the railroad skirts along the foothills. We arrived in Pueblo about 1 P.M. and found the firemen's tournament going on. As our train to Cañon City did not leave until 8 P.M., we thought we would look at a game of baseball between Pueblo and Trinidad. We spent our time loafing and writing until at last it became time to leave. During the afternoon I decided to go with Uncle Lewis to St. Louis to the meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. We arrived at Cañon, walked up to the hotel, got our rooms and went to bed.

St. Louis [Mo.], August 21, 1878.

We got up late the next morning, Wednesday, August 14, and changed our clothes for civilian's dress, during which performance Henry appeared. After buying a new pair of shoes, I went to breakfast, after which Donald, Percy, Henry and I got horses and started for Marble Cave. We had a nice ride to the cave, tied our horses to some trees and set to work picking up specimens of marble, etc. We next went into the cave. We had remembered to bring some candles. This cave is very deep and worth seeing. The marble is a species of red marble which when polished is very handsome. After staying for some time at the cave we arranged our specimens and started for home. On our way we saw a good many owls sitting on prairie dog holes. We got back just in time for tea, after which we packed our trunks, as we were to leave at 4:20 in the morning.

We were waked before light the next morning, Thursday, August 15. We went down to the train, got on board and started off. On the way we ran over the fore legs of a cow and so had to shoot her. We took breakfast at Pueblo and then started to Colorado Springs. The scenery on the way was about the same as from Alamosa to Pueblo. We arrived at Colorado Springs at 8:35 and took the stage for Manitou, 7 miles distant. On the way we passed Colorado City, once capital of the territory, and saw the old capitol, an old wooden barn now nearly falling to pieces. We arrived at Manitou about 10 A.M. and took rooms at the Beebee house. We boys made arrangements to walk to the top of Pike's Peak, for we had left Uncle Lewis in the train, he not caring to go to Pike's Peak. We started out about 10:30 A.M., taking a drink at one spring on the way up. We followed Fontaine Creek for six miles or more and then we came to the Lake House 4¾ (miles) from the summit. As it was cloudy and I did not see the trail, I made a mistake and went down the old trail to Colorado Springs. I did not find out my mistake until I had walked four miles. I then had to turn around and foot it back. In the meanwhile, Donald and Percy had walked on towards the summit. I caught up with them about two miles from the summit. We sat down and ate some lunch. It then began to blow very cold and to rain, and as it was very cloudy we decided to go back. We got back to the Lake House at 6 P.M. From here to Manitou, 8 miles, it took 85 minutes. We got back to the hotel a

7:30, tired out, hot, and dirty. We got some supper, cleaned up and visited the springs, after which we went to bed. The hotel is a very nice one although the prices are very high. The table is very good, the best we found.

We were waked at 6 A.M., Friday, August 16. We took our breakfast and then seats on the top of the stage. When we were seated, Percy pointed out to me Rose Keep, and sure enough, there she was in deep mourning. At the same time we saw her, she saw us as she was going down in the stage. She came up on top and we had a long talk on different subjects. On the way down, we could see a little of Cathedral Rock in the Garden of the Gods. We took our seats in the train, arriving at Denver at about 12. As Uncle Lewis had gone to the Wentworth House, we boys stopped there, while Mrs. Marshall Field,[11] Rose Keep, Miss Scott and the children went to another house. After dinner I paid a visit to Bishop Spaulding, but found that he was in Europe. I had a very nice conversation, however, with Mrs. Spaulding and Mr. Thomas, an assistant of his. We boys next went to see Mr. C. M. Henry, Supt. of the Colorado Central, from whom we got passes to Georgetown.

We took the omnibus to the station and got seats in the train. The road runs up the cañon of Clear Creek. At Golden we changed cars, taking the narrow gauge division. After getting some way above Golden, the cañon narrows very considerably and gives some beautiful scenery. I had a long talk with Rose Keep, going to Georgetown.

When we got there, we drove in an omnibus to the Barton House. I registered for the whole party. After supper, I enquired where Mr. F. M. Taylor lived, and then went to his house. I went into the Concentration and Excavation works[12] and saw them in operation. After I had seen the works, Mr. Taylor came in and I introduced myself. I then went back to the hotel and found it very late.

The next morning at Georgetown, August 17, I took leave of the boys, who were to stay over and meet me at Chicago. I took the train at 7:25. I saw the large rock near Forks Crk. called "Mother Grundy." On the way down the front wheels of the car I was in went off the track, but they soon arranged it.

When I got back to Denver I walked to the hotel and saw Uncle Lewis. After dinner I went down to the station and kept seats for Uncle Lewis and myself. Our train started at 3:15. After we had been gone a little while, Uncle Lewis introduced me to Judge Cobb, ex-Chief Justice of Kansas.

During the afternoon I bought some views and a few Indian traps, made by the Comanches of New Mexico. In the evening, Uncle Lewis went to the drawing room car, but I made myself comfortable in the common car. The scenery during the day was the same as we had seen on the A.T.&S.F.R.R. We went along the Smoky Hill River for miles and miles. At the beginning it did not have much water, but it gradually increased in size.

The next morning, Sunday, August 18, I busied myself preparing my paper for the meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. Judge Cobb amused us part of the day telling us stories and anecdotes. At Junction City we crossed the Republican River and saw it join the Smoky Hill River thus making the Kansas River. This part of Kansas is wonderful in appearance. The corn has grown to a height of 9–12 feet. The road runs along the north bank of the river. We arrived at Kansas City about 5 P.M. and immediately took our things into the Missouri Pacific train which left a few minutes after we got there. It soon got dark, so I arranged myself comfortably for a night's rest.

The next morning, Monday, August 19, we arrived in St. Louis and went to the Lindell Hotel. After breakfast we set to work on our papers. This kept us busy all the morning. In the afternoon, Mr. Bandelier[13] arrived from Highland. We had a long talk and then went down to parlor 22, reserved for the Association, and Uncle Lewis introduced me to Prof. F. W. Putnam,[14] Permanent Secretary of the Association. We then went out for a walk and Mr. Bandelier took a package to the train. After supper we went up to the room and prepared our papers. St. Louis is a very hot place, so much so that the perspiration would roll off you even when quiet.

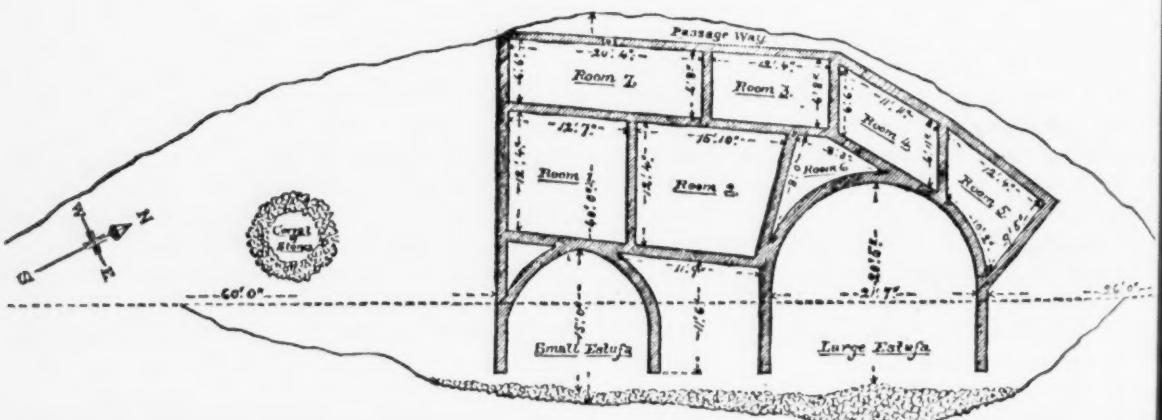
The next morning, Tuesday, August 20, I woke up late and after breakfast I went with Uncle Lewis to parlor 22. As we were coming out we met Prof. O. C. Marsh[15] of Yale. Uncle Lewis introduced me to him. He had just got back from Europe. We then went upstairs and went on with our papers. After dinner, Mr. Bandelier, who had gone out to Highland, came back and we went for a walk to see Prof. Henry A. Ward's[16] plaster casts which he has for sale. We did not see Prof. Ward at the store, but Uncle Lewis and Mr. Bandelier saw him later. In the evening Uncle Lewis introduced me to Prof. A. R. Grote[17] of Buffalo who I think is a very nice man. In the evening we wrote some more of our papers.

The next morning, today, we woke up earlier. After breakfast I went out and bought some draw-

ing paper, etc. I then went upstairs and continued writing until nearly 10 o'clock, when we went to the opening meeting. Uncle Lewis introduced me to Dr. Dalrymple of Baltimore. After the general session was over, the meeting adjourned to meet in sections. After that meeting was over, I waited for Uncle Lewis who, being elected to the Sectional Committee,[18] had business after the meeting. I was now a member of the Association, as I was elected in the morning. After dinner I went on with my work for a while until Prof. Grote came up and asked us for our papers to write an abstract for the *New York Tribune*. I then went down to parlor 22 and while he was at work I drew my ground plan and made an abstract of my paper. After tea I worked a little and then we all went to Washington University to hear the addresses of the two vice presidents, Professors Grote and Thurston.[19] After the proceedings were over, as there was a general invitation to go to Schnaider's Garden and the omnibuses were at the door, we all got in. Our party was Uncle Lewis, Prof. Ward, Mr. Bandelier, and myself. We walked around a little while and then sat down at a table and ordered some beer. This garden gave us a good insight into beer gardens in Germany, women, even, drinking beer. We enjoyed it very much.

We got up pretty early the next morning, Thursday, August 22, and I finished my ground plan. We then went down to breakfast and sat at Prof. Marsh's table. After breakfast we went to our room and finished our papers. Mr. Bandelier then came up and made an abstract of Uncle Lewis's paper which I wrote. We then went downstairs and hired a carriage in which Uncle Lewis, Prof. Grote, Mr. Bandelier, and myself drove to the Washington University. The first thing I did was to procure my member's ticket, after which I went up to the meeting of the Association in the chapel. After some more general business the Association met in sections as on the day before.

The first paper read [in Section B] was by Mr. Henderson of Springfield, Illinois. It was on some excavations in mounds near Naples, Ill. He had with him some beautiful specimens of pipes, copper axes, etc. used by the mound builders. His paper was very interesting and called forth a good deal of comment. Uncle Lewis raised a point that the mound builders were not ancestors of the Indians, in which he was supported by Prof. Marsh. After this paper there were several down on the programme which were omitted. Then came a paper by Prof. Riley[20] of Washington. It was an entomological paper and so I did not understand it. Besides, as my paper came soon, I was quite frightened and did not listen much. Next came a short paper and after it a recess of ½ hour. During this recess I had a chance to put up my diagram and collect my thoughts, for I was still frightened. At the end of the recess, the chairman came in, called the section to order and announced my paper, "Description of a Cliff-House in the Cañon of the Mancos River, with a Ground Plan of the Structure."[21] I was now very frightened, for this was my entrance into the arena of a scientific life. You can imagine my condition from the fact that I forgot to say "Mr. Chairman" at the beginning of my speech. I took my position by the platform and began in a loud voice, for I had not lost it. I soon regained confidence and read my paper as if I had done nothing else all my life. I referred every now and then to the plan. After reading 15 or 20 minutes, I finished and went back to my seat. My audience, which was quite large, applauded me, which made me feel much better. Prof. Putnam, Major Powell,[22] Mr. Mason,[23] and Mr.



Ground plan of cliff-house, drawn by W. F. Morgan for his paper presented at AAAS meeting in 1878.

Henderson made remarks on my paper, and then Mr. Bandelier made some remarks, after which Vice President Grote, Chairman of the Section, said, "We have all listened with pleasure to the paper of Mr. Morgan, and compliment him on it. We also wish to congratulate ourselves on the advent among us of another anthropologist who may follow the footsteps of his illustrious uncle." These remarks called forth some applause. Thus ended my first appearance on the scientific stage, a half hour which I shall never forget; I feel so proud of it.

Next came two papers by Uncle Lewis[24] which were afterward commented on, and then came Mr. Bandelier's paper.[25] It was a very interesting paper, but owing to want of time, he could not finish it. It is a paper of great importance and when in print will be of much value to ethnology. After the meeting I went with Uncle Lewis to the hotel and took dinner at Prof. Marsh's table. After that I went to my room and packed, for Uncle Lewis had decided to leave that same evening. We went downstairs and met Major Powell, who congratulated me anew. Uncle Lewis then had a long talk with Major Powell, who then introduced us to Capt. Dutton.[26] We then went to a beer saloon. Our party was Uncle Lewis, Major Powell, Capt. Dutton, Mr. Bandelier, Marcus Benjamin,[27] and myself. We staid at the beer garden a little while and then walked back to the hotel. Major Powell tried to induce Uncle Lewis to stay, but in vain. Prof. Marsh then came downstairs and he tried too, but with no better success.[28] We had made an engagement to go to Dr. Engelmann's[29] house at 5:30, but this we had to break. So, after taking tea, we had another talk with Major Powell. Prof. Marsh then wanted us to go up to his room and see some pottery from Chirique, Central America, but it was too late. Prof. Ward had engaged to meet us at the Depot, so we found him and Mr. Howell, his brother-in-law, at the station. We took the C&A train at about 8 o'clock. We left Prof. Ward at Main St. Mr. Howell staid with us. During the night I had a long talk with him.

We traveled all night and arrived (at Chicago) Friday, August 23. As it had been agreed that I should stop at Chicago to wait for Donald and Percy while Uncle Lewis went on to Rochester, I told Uncle Lewis goodbye and took the Omnibus to the Grand Pacific Hotel. During the morning I busied myself writing my dairy, etc. In the afternoon I went to the Chicago Exposition Building where Prof. Ward has placed his "Mastodon." I went upstairs and took a good look at it and then came back to the hotel.[30]

NOTES

1. Conejos is the oldest existing village in Colorado. The original settlement in this vicinity was Guadalupe, on the opposite side of the river from Conejos. According to local tradition, a pack mule of a Spanish traveler balked there. After ordinary means had failed, the owner vowed to build a church on that spot dedicated to the Virgin of Guadalupe if the mule would move on. It did. The vow was fulfilled and the village of Guadalupe grew up about the church. Because of recurrent floods, however, the inhabitants moved to higher ground across the river. The name of this village became Conejos, but its church is still the Church of Our Lady of Guadalupe.

2. Lewis Henry Morgan is called "Uncle Lewis" in the diary.

3. Drummers: traveling salesmen.

4. Charles Bent (1799–1847) was an early western pioneer. In 1835, at Fernandez de Taos, he married Maria Jaramillo whose younger sister later married Kit Carson. General Kearny, after his bloodless conquest of New Mexico, appointed Charles Bent civil governor on September 22, 1846. The following January, Governor Bent and several others were assassinated in an uprising at Taos. Major Price led an expedition against the Mexicans and Pueblo Indians involved, and quickly subdued them.

5. See *House Life*, pp. 144–153, 182–183.

6. Major Lafayette Head fortified and successfully defended Guadalupe (see Note 1) from the Utes in 1855. When Colorado became a state in 1876, he was elected its first lieutenant governor.

7. Bullwhackers: drivers of oxen.

8. General Heffernan, see *House Life*, p. 188.

9. A short time before the arrival of the Morgan party, the houses, stores, and churches of Garland City had been moved on flat cars to Alamosa. They had been set up and occupied in a few days, but work on them was still going on.

10. When he was writing his diary the next day, evidently William Fellowes Morgan could not remember the Indian name of the Spanish Peaks, so it is omitted in the diary. The name Huajatolla has been inserted by the editor. This Spanish-Indian name means "Breasts of the World" rather than "Twin Breasts." The twin peaks rise to heights of 12,683 and 13,623 feet, respectively, and are quite a distance from the Culebra Range of which they are a part. They served as landmarks to guide the early explorers. The Utes believed that the peaks were inhabited by evil spirits.

11. Nannie Scott Field, the first wife of Marshall Field. In the latter part of the diary, Mr. Morgan tells about his visit to their home in Chicago.

12. The settlement of Georgetown, Colorado, began with the discovery of gold there in 1859. The placer mines soon gave out and a period of stagnation followed. Shortly afterward, however, Georgetown became the most important silver mining camp in Colorado and remained so until the great Leadville silver

strike occurred in 1878. Naturally, W. F. Morgan was interested—he later specialized in mining.

13. Adolph F. Bandelier (1840–1914), archeologist and specialist in Spanish-American history. A biographer says of him, "No American archeologist has depended as did Bandelier on historical sources and no American historian has checked his historical work so carefully by a study of archeological materials." See *House Life,* p. 84.

14. Frederic W. Putnam (1839–1915); assistant of Louis Agassiz, 1856–64; archeologist; Curator of Peabody Museum; Permanent Secretary of AAAS, 1873–1898; President of AAAS, 1898.

15. Othniel C. Marsh (1831–1899) head of expedition in 1870 to study cretaceous and tertiary fauna in the west, presented his great collection to Yale, January 31, 1878; President of AAAS in 1878.

16. Henry A. Ward (1834–1906), student of Agassiz, best known for his geological collections and for the exact replicas he made of rare fossils and animals. The "Ward Cabinets," usually containing both natural specimens and replicas, were very popular, selling at prices up to $100,000.

17. Augustus R. Grote (1841–1903); entomologist and Curator of the Buffalo Society of Natural Science; Vice President and Chairman of Section B of the AAAS in 1878.

18. At that time the AAAS contained only two sections: Section A—mathematics, astronomy, physics, chemistry, mineralogy; Section B—geology, zoology, botany, anthropology. There were however, three subsections C—chemistry, D—anthropology, E—microscopy. Lewis Henry Morgan was chairman of Subsection D in 1875–76. At the 1878 meeting, although one-third of the papers of Section B were in anthropology, it was decided not to subdivide Section B, so Subsection D did not organize.

19. Robert H. Thurston, Stevens Institute of Technology, Vice President and Chairman of Section A of the AAAS in 1878.

20. C. V. Riley, U. S. Entomologist.

21. William Fellowes Morgan's paper is published in full (with the diagram of the ground plan of the cliff-house) in the *1878 Proceedings of the AAAS* (1879), pp. 300–306.

22. Major John Wesley Powell (1834–1902); Director of the U. S. Geological Survey. He led the first expedition through the Grand Canyon of the Colorado in 1869.

23. Otis T. Mason, Columbian University (now George Washington University). He presented two papers on Indian language and antiquities.

24. The two papers presented by Lewis Henry Morgan were: "Remarks on the Ruins of a Stone Pueblo on the Animas River, New Mexico, with a Ground Plan" and "Observations on the San Juan River District as an Important Ancient Seat of Village Indian Life." The contents of both of these papers are included in *House Life,* on pp. 172–188 and 192–197, respectively.

25. A. F. Bandelier's paper, "On the Sources for Aboriginal History of Spanish America," is published in full in the *1878 Proceedings of the AAAS* (1879), pp. 315–337.

26. Clarence E. Dutton (1841–1912); geologist; member of U. S. Geological Survey of the Rocky Mountains; published important findings on the nature and speed of earthquake vibrations .

27. Marcus Benjamin; graduated Columbia School of Mines, 1878; became editor of the publications of the U. S. National Museum.

28. Had President Marsh known of the telegram he was to receive the next morning, he might have had "better success." At the general session on Friday morning, August 23, occurred the high point of the meeting. After Thomas A. Edison (elected Fellow at this meeting) had been introduced, President Marsh announced to the members of the AAAS and *to Edison* that he had received by telegram that morning the welcome news that the Grand Prize of the Paris Exposition had just been awarded to Thomas A. Edison "for one of the most wonderful inventions of the age." Edison's newly invented phonograph was exhibited for the first time at the 1878 Paris Exposition.

29. George J. Engelmann of St. Louis, "venerable" botanist and one of the charter members of the AAAS, presented a paper and was elected a Fellow at this meeting.

30. William Fellowes Morgan remained in Chicago six days, meeting the boys there and leaving with them on August 28. The diary ends the next day.

Industrial Research as a Tool of Industry*

S. L. HOYT

The author has been engaged in industrial research for more than thirty years, the past fourteen of them at Battelle Institute, where he has specialized in problems of physical metallurgy. He is an authority on the selection of metals and alloys for engineering purposes. Dr. Hoyt has made numerous contributions to technical journals and is the author of several books and monographs, including Metals Data. *He received his B.S. in mining engineering from the University of Minnesota and did graduate work at Charlottenburg (Germany) Technical School and at Columbia University, where he received his doctorate.*

I FIRST MET Dr. G. K. Burgess on July 22, 1912, in Washington, D. C., and came to know him as a very warmhearted and wholesome person. As Chief of the Division of Metallurgy, he had the high ideals of the true scientist, and the ready acceptance of contributions from his laboratory showed that he lived up to those ideals. Later, in his capacity as Director of the National Bureau of Standards, he applied his talents to a broader field, and the nation suffered a great loss with his death in July, 1932.

As I now recall it, a large share of his work was done in order to give reliable answers to basic questions of metallurgy. In this way, he provided others with a firm factual basis for handling problems that arose in their work. He felt that by advancing and perfecting basic metallurgical knowledge he could contribute to the improvement of the technology of the metal arts. I also recall that he had a fine appreciation of practical research of the almost purely industrial type.

As I have just said, Dr. Burgess believed in what I am calling Industrial Research and participated in it. This particular applied type of research has become a great national asset and is now the almost universal method for improving existing technology and for putting to use the findings of science. There should be no need to point out what technology means to industry and hence to the economy and prosperity of the nation.

Not everyone agrees with this viewpoint, however, and before proceeding to a positive appraisal of my subject, I should like to dispose of the negative view. A man who for years has been in the upper circles of our government has stated it as his belief that the country would be very well off if technology were frozen, with no further research and development, and no additional applications of science in a practical way. This is an amazing statement, coming from an intelligent man; although it is also a challenge to the validity of the system that has advanced our material civilization so remarkably. What is the rebuttal?

Upon thinking it over, I could see no reason why the statement applied specifically and uniquely to the time that it was made; it could have been made just as appropriately the preceding year, or ten years earlier, or one hundred years earlier. At the end of my regression, I found that I was back to Adam and Eve. You may recall that they lived under the simplest possible conditions, with apples for food, fig leaves for clothing, and nothing to worry about except a serpent. Possibly this is an oversimplification, yet I suppose there are many who think that no further technological development is desirable. If a highly educated and intelligent man could feel that way about material progress, I felt that I had to justify my own belief in it. Intuitively I felt that the gentlemen was wrong and that what man has accomplished technologically to improve his working and living conditions, his health and leisure, and his cultural and recreational pursuits, is good in the most devout sense of that term. The fact that we first desire and then enjoy these things is, to me, a powerful argument in their favor. However, I chose not to go back to Adam and Eve to justify my belief in technological development; I settled for a century, and it is interesting to contrast the situation as it then was with what it is today.

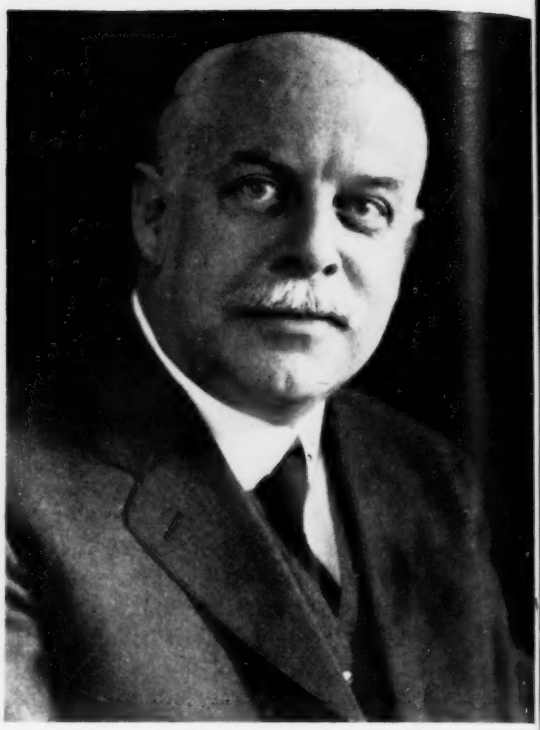
Not many more than one hundred years ago the first crude railroads were being built. The inhabi-

*Burgess Memorial Lecture, American Society for Metals, Washington, D. C., February 9, 1953.

tants had hardly become used to the wonders of the new canal system when steam power revolutionized transportation on both land and water and greatly extended the factory system of manufacturing. That, it will be recalled, was the period of the rise of the Industrial Revolution. Presumably, this learned gentleman would have frozen technology at that time, too. If so, he would deprive the worker of the benefits of mechanical and electrical power and would leave the family unit largely dependent upon its own resources with its activities limited essentially to the sphere of the backwoodsman.

It is interesting to note what another man had to say of that same system; yet this was said in the year 1828. This man was likewise learned, but he was also enlightened. I refer to the German poet, Goethe. In one of his novels, *Travel Years of Wilhelm Meister,* he considers the impact of the Industrial Revolution on society. At that time there was great agitation against the new order; the cry was to the effect that the rich will get richer while the poor will become slaves or be thrown out of work. In this book, Goethe takes time out from telling about Wilhelm Meister (himself) to describe life in a new, though fictitious, factory town. The inhabitants were weavers, but, instead of doing handwork at home, they operated power looms in the factory. Here came the vision. The product of those looms was of good quality and relatively inexpensive; hence the owner readily located markets among others who were likewise enjoying better remuneration for their services. Out of this exchange there grew trade and commerce which gave many people better employment than they had previously known. They all benefited because they could then enjoy a standard of living that was formerly beyond their means. In this way, Goethe presented a picture of the benefits of industrialization in terms of an improved standard of living. Personally, I am willing to let Goethe provide the rebuttal; we can profit today from the thoughts of this great man on social and management problems.

This is perhaps a good time to define a little more closely what is meant here by industrial research. First, it is research, an investigational discipline that is pursued in order to uncover new facts or principles. Research is usually classified as basic and applied. Industrial research is mostly of the applied type because there is an immediate and practical use in mind which someone is more or less patiently waiting to apply. Basic research is commonly done merely for information's sake and there is seldom anyone waiting to use the findings,



Dr. G. K. Burgess

patiently or otherwise. Industrial research may also avail itself of basic research, however; a good example is the current work on titanium-alloy diagrams. I think we may say that basic research gives us the seeds which industrial research then plants and nourishes, whereas industry produces the fruit. Obviously, without the seed no fruit will be forthcoming; but, aside from paying my respects to basic research, I shall concentrate on the applied type.

I wish to emphasize that industrial research is something new. Whereas science and scientific research grew up together, that was not true of technology and industrial research. For many centuries, technology advanced by clever inventions and by the lessons of experience. The history of cathedral construction illustrates this point very clearly. Thus, building one house taught how to build another one better. From the memoirs of an elderly engineer, I recently read that his firm erected its shops in 1880 by just such rule of thumb methods. They used the committee procedure of taking mutual counsel. Furthermore, there was not an engineer on the staff; today, however, a firm in that type of business would have both engineers and laboratories.

To get along in history to the start of industrial research, we come to relatively recent times. I do not know whether the Schenectady laboratory was

the first full-time industrial research division in this country, but it was surely a bold pioneering step a bare fifty years ago. For, although the Industrial Revolution was then far advanced, management in general was not yet ready to adopt the experimental method in a business enterprise. On the other hand, it is also true that scientists failed to visualize industrial research as a career; in fact, it took several years of commuting between Boston and Schenectady before Dr. Willis Whitney could make up his mind about the nature of his life's work. Since this attitude of management toward research is important for an understanding of the historical development of my subject, I should like to quote a pertinent statement of a very learned man, Max Planck, that bears on the factor of the human element. He stated: "An important innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul.* What does happen is that its opponents gradually die out, and that the growing generation is familiarized with the ideas from the beginning."

Let us pause a little longer at the turn of this century, the period of the inception of industrial research. The old system of technological development then in current use had already given us railroads, steamships, electric street cars, the electric arc and incandescent lamps, electric power, the telephone and telegraph, the phonograph, gasoline and Diesel engines, automobiles, farm machinery, machine tools, the Bessemer and open hearth processes of steel making, numerous steels and alloys, etc. Compared to a century previous, use of the primitive methods had advanced material civilization tremendously. But let us take note of the scientific discoveries of that period that awaited development and use. Steinmetz was at work on alternating-current theory and equipment and on the long-distance transmission of electric power. Roentgen had discovered his mysterious x-rays and Hertz his curious electromagnetic waves, while Becquerel had discovered natural radioactivity, and the Curies had isolated radium. There also was the baffling Edison effect which was to be harnessed for radio and the electronics industries while, in the field of metals, numerous rare ones had been isolated as laboratory curiosities and metallurgy had just rounded the corner as a budding science.

Beginning at about that time, the industrial use of the products of science was beyond the capabilities of the empirical and intuitional methods. The handwriting was on the wall, and here and there the especially enlightened managers saw the need of using science in applying science. As an example, I would like to cite the case of the electrical industry, in order to illustrate the contribution of the experimental method to the development and growth of industry.

I have mentioned the Schenectady laboratory and Dr. Willis Whitney, who is undoubtedly the dean of industrial research directors in our country. It was E. N. Rice, Jr., president, and Albert G. Davis, in charge of the patent division, who had the inspiration to have the General Electric Company enter into research on a formal scientific basis. It was also Mr. Rice who secured the services of Steinmetz and who, in general, saw that the electrical industry was in its infancy and in need of men of specialized scientific training. This movement was notably assisted by Elihu Thompson, who was brought into the fold by the combination of the Thompson-Houston Company and the Edison Electric Company, to form the General Electric Company. A little later, F. S. Terry and B. G. Tremaine established the National Electric Lamp Association. They were great organizers and managers, but our interest in them here comes from their recognition of industrial research as an essential part of their business. In those early days, that was vision indeed.

For the purpose of housing their home office and research and development facilities, they installed Nela Park in Cleveland, sometimes referred to as the University of Light. A small effort went into basic research on light and radiation, but it was to applied research that the greatest attention was paid. For this, they set up development and testing laboratories and a small or pilot plant factory. I think it is consistent with the vision that they displayed in other activities of their organization that they insisted that these research facilities improve incandescent lamps and the methods for making them in order to produce more light for less money. Your incandescent lamps have been doing that for all of you these many years as a symbol of what industrial research is capable of accomplishing when under good management.

The chemical industry is also a good example because, like the electrical industry, it had to have scientifically trained men in its employ. It has long been a leader in this field and has plowed back a higher percentage of its sales into research than have other industries. Then, during World War I, there came a powerful stimulus for research when the importation of chemicals from Germany

* This refers to the change in Saul who, while on the road to Damascus, gave up his early thinking to adopt the teachings of Jesus; whereupon his name became Paul.

was cut off. Furthermore, it was the chemical industry that introduced the first privately operated institute for industrial research. That was the conception of Robert Kennedy Duncan in 1907 which later became the basis of operations of the Mellon Institute in Pittsburgh.

The automobile industry did not lag far behind, possibly because it had the advantage of certain outstanding personalities, such as Ford and C. F. Kettering. Those men of vision led the way, while the terrific competition for the patronage of the purchasing public resulted in the survival of companies whose managers recognized the advantages of research and development for improving and reducing the cost of the product.

One of the miracles of that age was the development of the airplane by the Wright brothers. I mention it here although its classification is somewhat difficult. I think one would say that the work of the Wrights was invention and development, although they made use of engineering principles to guide them patiently through a maze of difficulties to reach their goal. It was surely experimentation, but not the systematic type of research in which significant variables are studied and their effects evaluated. Another reason for mentioning the work of the Wright brothers is to suggest that we should not let overenthusiasm for formal research blind us to the possibilities of advancing by other techniques.

Turning to the field of metallurgy, it was during the decade which included World War I and the early twenties that a conspicuous expansion occurred in the use of applied or industrial research. As I recall it, this was first noticed in the producing industries, particularly in their aluminum, zinc, and nickel branches, with some healthy signs of progress in the field of precious and rare metals. Not long thereafter, similar developments occurred in the copper, magnesium, tin, and steel branches, with cast iron and high alloys coming in shortly before World War II. The situation regarding lead is not clear, although it is known that this branch has supported considerable research.

The consuming industries form a heterogeneous group with great variations in size, etc., of the individual units and in facilities for doing research. Furthermore, in the past, they have been relatively slow in taking advantage of the benefits that come from research. The electrical industry is a consuming industry in metallurgy and it is only fair to say that it showed much less zeal in tackling its consumer problems than it did in developing its own new products. In the past, this has been true in numerous other instances. This is mentioned not as a note of censure, but in order to arrive at a reason for the tardy adoption of the research method by the consuming industries.

I have repeatedly run across the attitude of the consumer that his supplier will give him the information he needs for fabrication and even for his products when in service. Any company that is large enough to afford research, I believe, makes a grievous error with such a policy. This is not to be interpreted as a reflection on either the ability or the integrity of the supplier, but is intended, rather, to point up certain principles of the use of industrial research. For I believe that the consumer is in a position to know and should know more about the requirements of fabrication and use of his products than is possible for his supplier to know. I also believe that if the consumer acquires this knowledge, which will go well beyond that needed for purchase specifications, he can do a much more intelligent job of purchasing his own raw materials. He will know just what points he must insist on in procurement, both in and out of the specifications, and what points he can concede. The availability of this kind of information makes selling and purchasing a more intelligent process.

A few examples may bring out more clearly what I have in mind regarding the use of research in support of procurement. At one time we had the problem of purchasing a large amount of steel sheet for single-coating enameling. There was an obvious answer to the steel question—the purchase of the non-reboiling enameling quality steel such as is used for household appliances. However, the competition in our market was such that we knew we could not afford such expensive steel. After we had made our studies of steel behavior in fabrication and in enameling, we were able to tell our suppliers just what we needed. With their intelligent cooperation, we found that we could use a cheaper grade of sheet, and there followed a sizable and satisfactory business. We also got a new feeling for the maxim of the engineer that "good enough, is best."

Another experience likewise brings out the value of this use of industrial research, only more pointedly. This was a case of procurement of steel in large tonnage for an application which had strength as a primary requirement. During the preliminary or screening tests a certain steel was selected which carried an extra cost of $4.00 per ton. The reason for using this type of steel, however, was not its strength but to give our customers the quality needed for satisfactory performance in

ervice. That extra charge was a target to shoot at and so we went to work. We put into practice the principles that I have mentioned, and after considerable laboratory work and observation of fabrication, and with the close cooperation of the supplier, we learned that with a rather well-defined steelmaking practice we could secure the necessary quality at a cost of only $2.00 per ton extra. With large tonnage purchases, that saving paid for a lot of research. The supplier got the business. The moral here is that by systematically studying the steel variables and correlating them with behavior in manufacture and with tests of the quality of the finished product, we were able to effect these savings legitimately. It meant that we had to learn what the steel mill could supply, what we could do in our own shops, and what our customers had to have for satisfactory use of the product. The steelmaker could not be expected to acquire all that knowledge and, furthermore, we, and not the ultimate user, were his customer.

This use of industrial research as a guide to intelligent procurement is perhaps less commonly given as a function of the research laboratory. The more common uses for developing new products and processes and for selecting materials and their treatments are too well known to require detailed discussion here. However, practically every product and operation is a suitable subject for industrial research sooner or later, because science is forever making new contributions with possibilities for exploration, and technology is in a continuing state of flux.

The Federal Government has been a major factor in advancing technology and hence in promoting industrial research. There are numerous agencies involved and they purchase a wide variety of products, but they have one trait in common: they always want something better. As a naval officer once told me when I plaintively asked him how he thought we were going to make what he wanted, "We just give you the answers; the rest is up to you people." I also recall an experience at a steel foundry when I was doing some work during World War I. On my first trip there I found the superintendent fussing and fuming about having to work to government specifications. "Well," he said, "we've always just poured our castings and let them go. Now these government specifications call for pull tests and we're held down tight on elongation." I told him he would have to normalize his castings to refine the structure. Many steel foundries discovered "elongation" during that period, while during World War II, with the tight alloy situation, they were required to exploit liquid quenching.

Naturally, experiences of this kind could be repeated over and over. What they teach here is the influence which the military services exert on technology by always demanding more and more. With that kind of pressure, management is faced with increasingly difficult problems of devising the best technology to meet the situation. The recent and current problems of gun-barrel erosion, steel cartridge cases, cold extrusion, low-alloy steel armor plate, welding armor, steel quality for ship hulls, the alternate and boron steels, etc., point up the utility of research today. Even more illuminating is the recent work on the development of jet propulsion, the utilization of nuclear energy, and such materials as the super-alloys, titanium, and zirconium. It is inconceivable that such matters could be handled effectively if we could not apply the methods of industrial research. There are other cases which I might have used, some of which were shining examples of the intelligent use of industrial research and some definitely not so shining. Thus, there are the producing and refining branches of the oil industry, the gas-transmission industry, the railroad and transportation industry, the welding industry including welded structures, the pulp and paper industry, the printing trades, and agriculture. In all these cases, industrial research has been used to a greater or lesser extent, depending largely on the vision and progressiveness of management and secondarily on the potential benefits which might come from research.

The broad question of who should do the research has never been objectively analyzed, to my knowledge. On the other hand, the electrical industry waited for no one to do the things that needed to be done for its future growth. The same is true of the chemical and various other industries. On the other hand, and leaving aside industries which have been backward in helping themselves, there is another case of quite a different type. Again, I shall use an example with which I am familiar. For years, the petroleum industry wasted enormous quantities of natural gas by loss into the air, and this at a time when that fuel could have been profitably used in our industrial and residential areas. The question is, should it have initiated a research program to find out how to transport that gas economically to market? It would have paid off handsomely if such a program had been successful. Or, since it was a transportation problem, should the railroads have undertaken it? Of course they did not do so, nor did other agencies

which might have taken up this project out of self-interest. Actually, the solution came from the pipe industry. In the long run, management is wise not to go outside its own sphere. Yet I believe that this aspect of industrial research deserves more attention now than it has received in the past. Where the interest of a consuming company or industry is greater than that of the supplier, the former might well undertake research from which it stands to gain. That is especially true today, because there are university and private research institutes which can supply the facilities needed for practically every branch of technology.

It is now appropriate to look at industrial research against the backdrop of history to appraise its position in the scheme of things. I reiterate that whereas basic research resulted from man's desire to know more about his world and the way in which it functions, industrial research came from his desire to make a better world for himself. To see this interesting development in its proper perspective, one has to philosophize a bit, and I look at it as follows.

The lower animals differ from man in that they live according to a fixed pattern. The coelacanth fish that was recently caught off the coast of Africa, and which has been termed a "living fossil," still lived essentially as its ancestors did 150,000,000 years ago. The squirrels in my yard retain the living habits that squirrels presumably have always had. They desire no research laboratory because, regardless of their mental capacity, they remain satisfied with their mode of living. Just as it may be reasonably postulated that one of the early fishes was altered by fortuitous mutation and thus was able to come up onto the land, so it can be conceived that something comparable occurred in some primitive member of the ape family and produced the first man. That early progenitor had a distinctively human characteristic—more likely a combination of two characteristics—which was something akin to the desire to invent and to make living better and easier for himself. In response to this urge, he invented tools and fire, made clothing, learned how to provide shelter, and the like. It is significant that he has passed that trait on to us of the present generation. In the material world, the greatest chain reaction was not that of the atom bomb but the one that was initiated by that first uniquely human mutation and that has since been transmitted by inheritance and has been improved by selection.

In the field of industrial research, we cannot expect much more of prehistoric man. So let us leave him with his fire, his crude implements, and his wheels, and pass on to recorded history to follow the development of technology and research. Until the introduction of industrial research, there were three periods when man reached new heights of intellectual output. Although the contributions of the great thinkers of those periods profoundly changed all phases of human activity, and in spite of continuing developments in engineering and technology, in no instance did man devise the general experimental method for advancing technology.

Our first period was that of the early prophets of the Old Testament, of the Greek philosophers, and of the craftsmen and engineers of Rome. We are not concerned here with their intellectual triumphs or with their important contributions to religious and moral teachings, but with what they accomplished in technology. This, it appears, was limited to engineering structures such as roads, buildings, temples, ships, and aqueducts, certain engines of warfare, and the products of the crafts. We are, of course, indebted to them for some of the sciences, and the Greek philosophers developed the unique human function of abstract reasoning. It is beyond doubt that they were progressing in most ways, but those fields that concerned their material welfare were left to the engineers and artificers and were handled by the empirical method.

The next period was the late Renaissance, when Europe was swept with a wave of new thought that freed it largely from the frozen dogma of the past. It was then that the physical sciences got their start with the experiments of Galileo in mechanics and the work of Copernicus and Newton on the movements of celestial bodies. Shortly thereafter, Newton formulated his rules of procedure for solving problems of the physical sciences. Thus, at a relatively early period, it appears that science, at least, was off to a sound start in both methodology and philosophic guidance; but technology proceeded in its ancient and empirical groove. The well-known prophecies of Roger Bacon and the inventions of Leonardo da Vinci demonstrate man's capabilities at that stage of his development, but the invention of a method for making inventions still lay beyond him.

The third period began about the middle of the eighteenth century when the Industrial Revolution was ushered in with Watt's steam engine and when civil engineering became an independent profession. This started the first great period of technological development, which may be called the technological age because of the dominant role

of technology in changing material civilization. The continued development of the physical sciences, which spread from physics to chemistry and then to thermodynamics and physical chemistry, mechanics, electricity, and finally to metallurgy, should also be noted. Due to this concatenation of developments in science and technology, it was inevitable that the methods of the former should be used for improving the latter. This occurred late in the nineteenth century. The end of this period would be the time when science assumed the leading role in technology, both in development and in practice. As already indicated, this did not happen abruptly; we are actually dealing with a period of transition during which empiricism gradually faded out and was replaced by the scientific method.

In my example of the electrical industry, the start came about 1900. Curiously enough, in metallurgy, a start can be detected at an earlier date if we accept the use of controlled experimentation by industry as our criterion. As examples, I would cite the work of Hadfield in England and Taylor and White, and Sauveur, in this country, although one could note the work of pioneers in other countries as well.* It was during the 1880's that Hadfield developed his silicon and manganese steels. The soundness of his work, and the reliability of the systematic, experimental method are attested to by the continued use of his compositions and treatments up to the present day. Sauveur correlated laboratory work on structure with steel characteristics and technical practice in another early example of the utility of controlled experimentation in industrial work. It was also by this method that Taylor and White developed the analyses and heat treatments that are still reflected in high-speed steel technology. The end of this transition period may be thought of as occurring about 1940.

This date is difficult to establish because it is not a unique point in history, like a proclamation, but a point of consensus. I mean that it was about this time that industrial research was accepted by the consensus of management. Just as a consensus of public opinion does not usually lend itself to yardstick measurement, except perhaps by polls, it is likewise uncertain just when this date should be set. I am judging largely by two criteria. In 1940, there were war clouds on the horizon and our government had a large procurement program under way. Not only did this call for development work but the government also sponsored research programs on a large scale. This activity, by bringing in new organizations and training new research workers, demonstrated the possibilities of the research method on a sufficiently large scale to have a nation-wide effect.

My second criterion comes from our experience at Battelle which, we think, indicated a swing in that direction at about the same time. In other words, research was in the air in industrial circles. Management seemed to change from the attitude of, "Should we spend company money on such a speculative venture as research?" to the modern approach, "What research program best suits my company's needs?" Industrial research had come of age.

This historical discussion will have brought out, I trust, the long apprenticeship served by technologists in their constant endeavor to improve their arts and crafts. From prehistoric times down to our own lifetime, the evolution was slow and labored; because it was wrought by the empirical method, it was relatively costly and inefficient. Ultimately, and I believe inevitably, the strong hand of management replaced empiricism with the tool of industrial research.

I have not cited statistics nor have I tried to describe the methods and philosophy of industrial research, for its mechanics and its place in industry and in society are well known. However, I do not believe it is beside the point to say that industrial research is a great national resource and one of the most significant factors in the strength of our country. Nor is it out of place to say that, in use, it has brought the prosperity to the nation and the material comforts to the citizens that were in the minds of those who financed it and participated in it. It feeds strength and versatility into the industrial machine and there come out more and better employment for the workers, better living conditions, more time for recreation, and, in brief, a better life for all of us.

The great and dynamic force of technology produces change, and the change has been synonymous with improvement. An object lesson of its beneficence is the daily worker who provides well for self and family by performing even simple tasks in production. In our country, through technology, a large and increasing population has been able, not just to subsist, but actually to improve its lot. Indeed, the great truth of technology is that it has made it possible for prosperity to be spread throughout the land. At no other time in history

* A classic example would be the early work of Chernoff on critical points and the heat treatment of guns.

could this be done, and in no other country has it been done. Because of this, technology should be understood in all walks of life in order to make sure that it remains healthy and in a position to insure our continuing prosperity.

Only when viewed in this light do we see the role of technology and its junior partner, industrial research, in its true perspective. Without technology and industrial research, neither legislation nor financing nor management nor labor could produce the changes and provide the comforts that we now enjoy. Indeed, whereas all the latter are vital components of industry they would be as impotent as our backwoodsman, were their functions not implemented by those two partners. To the scientists who give us facts and principles and to the research workers who translate them into industrial operations, we should be very generous in our recognition of what they give to society.

Dr. Burgess was in the midst of this great movement as contributor and manager. As Director of the National Bureau of Standards, he had a broad field for his activities. In his quiet but effective way, he did much to introduce the present scientific age, especially by means of the great powers that lie in the hands of management.

SCIENTIFIC MANPOWER—1915*

We must undoubtedly hold that if a larger supply of talent exists than is discovered, developed and put to use that, since, as we have seen, it is so valuable when estimated in terms of social progress, we are dealing wastefully with talent. We are allowing great ability to go to waste since we are leaving it lie in its undeveloped form. Therefore one of the problems of the proper conservation of talent consists in finding a method of discovering and releasing this valuable form of social energy. . . .

We shall be wise when we realize the worth of our workable talent and so establish its working conditions that it may secure the full measure of its productiveness. If scientific management for the mass of laborers of a nation is worth while how much more serviceable would it be to extend its fructifying influence to the most able members of the community.——The author (1866–1949) entered the field of sociology from the ministry. He received his Ph.D. at Chicago Theological Seminary and was a rural preacher in the Middle West. He later took his Ph.D. degree in sociology. In 1908 he established a department of sociology at the University of North Dakota and served as its head for forty years.

* From *The Conservation of Talent Through Utilization,* by John M. Gillette, THE SCIENTIFIC MONTHLY, I, 151 (1915).

Warm-Bloodedness*

SIMON RODBARD

The author, Assistant Director of the Department of Cardiovascular Research of the Michael Reese Hospital in Chicago, received his Ph.D. in 1941 from the University of Chicago, and in 1951 he received his M.D. from the University of Illinois School of Medicine. During World War II he served as Aviation Physiologist in the Air Corps. While stationed in the mountains of Arizona, he developed an interest in historical geology. At present he is engaged in studies of hardening of the arteries, and in the application of hydrodynamics to the understanding of the structure, function, and diseases of the heart and the blood vessels.

ON a bitter wintry Chicago night, with the thermometer hovering at sixteen degrees below zero, an obscure woman, benumbed with alcohol and with cold, stumbled into an alleyway and fell into a deep sleep. While she slept she became the object of the intense interest of physicians and research workers who recorded her every heart beat and respiration. Each half-hour the radio broadcast reports of her condition, and newspapers all over the country and as far away as Japan headlined her story. When she regained consciousness twenty-four hours later she was already assured of distinction in medical annals, as she had recovered from one of the lowest body temperatures (65° F) ever recorded in a human being.[1,2]

During this period of low body temperature, her heart had slowed to fifteen beats per minute, her respirations were three per minute, and the blood flowed sluggishly through her arteries and veins. Her blood pressure could not be determined. The circulation through her arms, legs, and internal organs had virtually ceased, and the total output of her heart had gone to supply only her heart, lungs, and brain. As her body temperature began to rise under the watchful eyes of a corps of physicians, her heart rate and respirations increased, the circulation of blood accelerated, and the blood pressure climbed back to normal levels.

The keen interest of medical science was due to the fact that the case of the "Frozen Woman" offered lessons on the ability of the human body to function far below the 98.6° F considered to be normal. Military medicine was anxious to learn what it could from the case in order to be able to improve the treatment of freezing and frostbite in the field. The recovery of this subject from severe hypothermia, and the recent intensive experimental studies on induced hypothermia served to accelerate the utilization of "artificial hibernation" in cardiac surgery. Public interest was in the miraculous persistence of life and the recovery from such unbelievably low body temperatures.

Biologists, divorced from the restrictive anthropocentric view, were interested but not astonished. Some of these scientists viewed the case as an atavism revealing the long-forgotten function of hibernation which certain of the more primitive mammals still exercise. The long march of the warm-blooded animals to a higher degree of freedom from the thermal limitations of their environments has occurred as a result of the development of a large variety of physiological mechanisms that have masked the hibernating function The history of this escape from control by the environment is recorded in the temperature-regulating centers and reflexes originating in the central nervous system. The story is elicited in the analysis of the phylogenetic evolution of the vertebrates, and in the study of the poorly developed temperature regulation of the newborn.

In the course of the evolution of the higher animals and probably of the higher plants and the insects as well, there has been a progressive increase in the ability to withstand rapid environmental temperature changes. It is known that fish are relatively sensitive to slight variations in their environmental temperature, and that a rapid change of a few degrees may lead to serious injury or death.[3,4] However, the ability to adjust to rapid temperature change is not essential for the survival of fish living in large bodies of water, since such changes do not occur in their environment (Fig. 1). Their ability to move to the surface or descend to the cooler depths apparently is adequate to meet thermal stresses produced by warming of the surface

* From the Cardiovascular Department, Medical Research Institute, Michael Reese Hospital, Chicago, Ill. This department is supported in part by the Michael Reese Research Foundation.

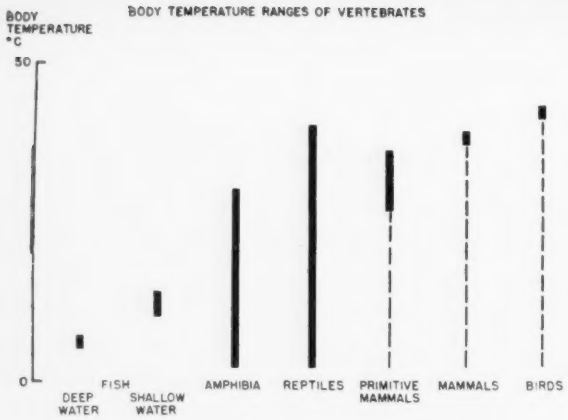


FIG. 1. A diagram to illustrate the progressive increase in temperature tolerance with the evolution of warm-bloodedness. The vertical bars represent the degree of tolerance to rapid temperature change. The dotted lines represent potential tolerance to thermal change that may be masked by development of temperature-regulating mechanisms. From SCIENCE, **108**, 413 (1948).

during the day. Shallow-water fish may have to adjust to somewhat wider ranges in the course of a single day and these forms, including the common goldfish, are known to withstand a somewhat greater degree of temperature change without injury.[5,6]

The increasing ability to tolerate rapid environmental temperature changes must have been a factor in the development of land-living forms from the shallow-water crossopterygian fish of the Devonian times of 340 million years ago. When these fish left the water to live on the land as the earliest amphibians, they were exposed to more rapid and wider swings in body temperature than could ever have occurred under the cover of the high specific heat of the protecting water. Within the course of the following 100 million years, some of these amphibians developed the capacity to tolerate even greater temperature ranges; these were able to abandon the water completely, and gave rise to the reptiles.

It is well appreciated that variations in the environmental temperatures occurring during the course of the day in temperate climates play an important role in the cycles of activity of cold-blooded animals living on the land.[7-9] During the day, absorption of warmth from the sun raises the body temperature and increases the metabolic rate (Fig. 2). Increases occur in oxygen consumption, in respiratory and heart rates, in blood pressure,[10] in the velocity of the circulating blood, and in general activity. (A similar sequence of physiological events was seen during the rewarming of the "Frozen Woman.")

Cold-blooded animals may regulate their body temperature at optimal levels by moving from sunshine to shade, or by changing positions in order to expose greater or lesser surface area to the sun. These functions are commonly observed in lizards and turtles basking in the sun.[9] When optimal temperatures are attained, those functions having to do with defense, nutrition, and reproduction are consummated; this results in individual and species survival. With evening, these cold-blooded animals, without ability to retain heat, lose warmth to their environment. This leads to a slowing of metabolism, of circulation, and of general activity: the animal may fall into a sleep-like torpor. Each day this cycle of warming and activity, cooling and torpidity, is repeated (Fig. 2).

The advantages of a body temperature maintained at high levels, independent of changes in the environment, readily become apparent. Animals capable of remaining warm and active obviously can dine at the expense of their cold and torpid neighbors. Natural history records numerous more or less successful attempts to maintain a high body temperature. This phenomenon is seen in the communal hive-warming technique of the bees[11] and in the warming-up preflight process in insects[12] and bats.[13] It is possible that these and other species are even now evolving in the direction of better temperature regulation.

Among the fossils of the Permian period of 220

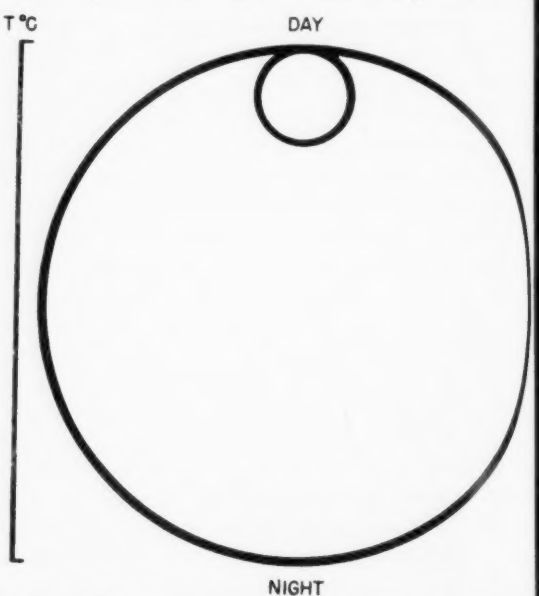


FIG. 2. The diurnal cycle. The large circle represents the body temperature changes during the day and night in cold-blooded animals living on the land. The small circle represents the diurnal cycle of warm-blooded animals. It mirrors the larger cycle but is limited to a narrow range.

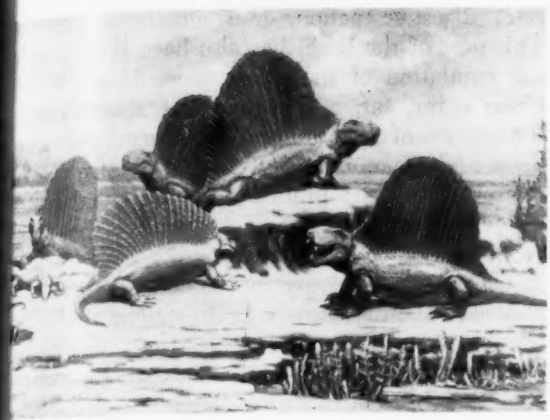


FIG. 3. Permian scene, by the late Charles R. Knight. Two species of reptiles with elongated vertebral spines are shown. The carnivore, *Dimetrodon,* is represented by four of the large reptiles shown. *Edaphosaurus,* an unrelated herbivore, has apparently wandered unhappily into the scene at the left foreground. (Courtesy of the Chicago Natural History Museum.)

million years ago, several species of reptiles, closely related to the ancestors of the mammalian line, present evidence for a unique temperature-regulating mechanism. These reptiles had long vertebral spines extending two or three feet above the middle of the back, and forming the skeletal basis for a sail-like structure (Fig. 3). While no definitive proof can be presented for the function of these sails, one interpretation[14] is that the placing of the sail perpendicular to the rays of the sun in the early morning would provide a greater surface area for the absorption of heat, thus warming the animal and bringing him to an optimal working temperature ahead of his sail-less cousins. Evidence in support of this interpretation is seen in the extremely large grooves in the spines that may have contained blood vessels. Such a mechanism would be of particular value in a period of relative frigidity such as the Permian is believed to have been. That these species were dominant in the land for nearly 10 million years speaks eloquently for the excellence of adaptations to their environment that may have included such thermic adjustments. The disappearance of these species in the succeeding warmer Triassic period suggests that the "heat collecting" sails may have finally become a liability.

It is in the Triassic rocks formed 200 million years ago that the paleontological record first shows evidence of the evolution of the early mammals. A kind of passive warm-bloodedness may have already been achieved by the dinosaurs. Their great bulk and relatively small surface area may have operated to keep them warm during the cold night. During this period the theriodont reptiles, which probably already had developed an ability to withstand large diurnal temperature changes, gave rise to the early mammals. Thirty million years later, in the Jurassic, an unrelated group of reptiles gave rise to the ancestors of the modern birds.

The more recent evolution of the birds, with the establishment of a higher body temperature[15] than that of the mammals, has given our feathered friends certain advantages over us. Their higher metabolic rate provides them with a more rapidly responding nervous system than that of their plodding mammalian cousins. This advantage could have led to the displacement of the ruling mammals and the avian mastery of the Earth. However, the birds did not develop a cerebral cortex adequate for this task. Instead, they specialized in the development of the basal ganglia of the brain, structures concerned primarily with complicated locomotion. Thus it appears unlikely that the birds will assume control of the destiny of the Earth, except perhaps by radioactive default.

The evolution of hair or feathers probably played an important role in the ability to retain warmth during the evening and night. Evidence for such a nocturnal role for the early mammals has been given in comparative studies on the eye.[16] The development of other heat-conservation mechanisms, such as the ability to erect the hair and to shift blood from the skin, served to increase the thickness of the layer between the cold external world and the warmer internal structures (Fig. 4). The development of a thick subcutaneous fat pad also helped to retain heat. Mechanisms increasing the production of heat by shivering and perhaps by thyroid stimulation completed the process of making these animals relatively independent of their environmental temperatures.

Because of the high body temperatures we maintain throughout the twenty-four hour cycle, it might be considered that, Joshua-like, we have made the sun stand still so that we might live out our lives at the high noon which our cold-blooded relatives can enjoy for only a few hours each day (Fig. 2). This high and constant body temperature is important not only for ourselves but also for the parasites who have come to live with us. Thus, the malarial[17,18] and filarial[19] protozoans, the pinworms,[17] and the body lice[20] are highly attuned to slight diurnal changes in the temperature of our bodies. This is evident in their responses to these variations.

The increasing efficiency of the heat-conservation and heat-production mechanisms finally brought about a new situation: the danger of rais-

ing the body temperature to critical levels. At this time, mechanisms resulting in a loss of body heat began to have survival value. These included reflexes that shunted the warmer internal blood to the surface; sweating, panting, and such positional changes as might reduce the intake of heat and increase the surface area exposed to the environment. Similar time patterns in the development of heat-conservation and heat-loss mechanisms may be seen in newborn mammals and birds: a recapitulation of the evolutionary process.

Warm-blooded animals long have been known to have extraordinary thermostatic centers in the brain that carefully regulate the body temperature within a fraction of a degree. Recent studies have shown that these temperature-regulating mechanisms of warm-blooded animals reside in a part of the brain known as the hypothalamus.[21] This minuscule portion of the brain has been shown to be a basic center for the regulation of a large number of apparently unrelated functions. The hypothalamus has been considered to be the central organ for the control of the entire autonomic nervous system, which in turn controls many of the unconscious functions of the body such as heart rate, digestive action, and glandular secretion. This part of the brain has also been implicated in the regulation of the blood pressure, respiration, blood sugar, fat, and water, and also related to the control of appetite and the diurnal rhythm of sleep and wakefulness.[21] Through control of the master gland, the pituitary, the hypothalamus is believed by some workers to control the sexual cycle.

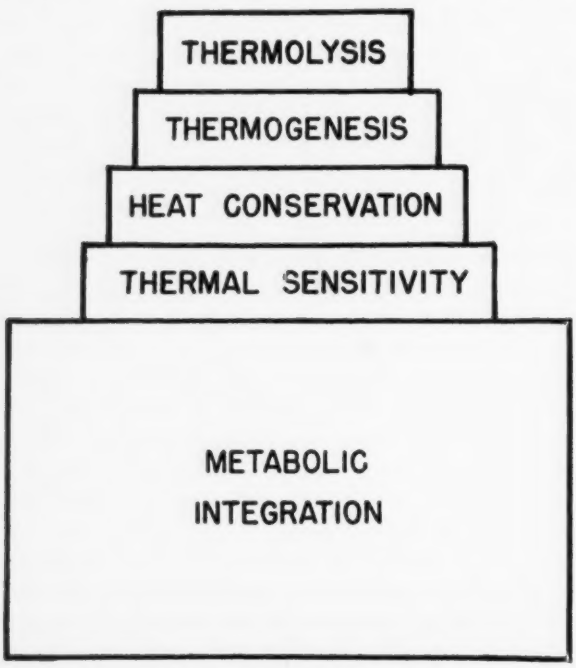


FIG. 4. Schema to illustrate the development of warm-bloodedness. The apparatus for Metabolic Integration of the organism provides the foundation for the development of Thermal Sensitivity. Utilizing this function, Heat Conservation apparatus develops. The later development of heat-producing apparatus (Thermogenesis) and heat-loss mechanisms (Thermolysis) completes the machinery for maintaining the normal body temperature of warm-blooded animals.

The recent discovery that cold-blooded animals also have thermosensitive brain centers that affect the blood pressure and other functions[22,23] has provided a basis for a better understanding of the evolution of temperature regulation, and of the bodily changes that are brought about under the stimulus of alterations in body temperature.[24] In these experiments the brain of the turtle was subjected to thermal stimulation. A wire connected to a water reservoir was inserted in a selected site in the brain. When warm water was placed in the reservoir, the brain surrounding the wire was heated and the blood pressure was seen to rise within a minute or so. When cold water was used and the brain was cooled, the blood pressure fell within the same time period. Attempts to localize this temperature-sensitive site have shown the most sensitive region to be in the same general area of the brain that is concerned with the regulation of body temperature in the warm-blooded animals.[25] Prior to this, it had been generally accepted that, unlike the warm-blooded animals, cold-blooded animals are completely at the mercy of their environmental temperatures because of the lack of adjustment mechanisms. However, the evidence that cold-blooded animals may make certain adjustments in their internal functions in response to stresses placed by the environmental temperature, shows that the beginnings of thermal independence are already present in these more primitive animals.

On the basis of the evolutionary approach discussed above, the apparently variegated and unrelated functions of the hypothalamus may be considered as parts of an integrative mechanism playing a role in the regulation of the internal environment of the body in response to changes in body temperature. It is apparent that as the amphibian leaves the water and is exposed to large changes in body temperature, enormous variations probably occur in the metabolic rates of the various tissue and organ systems. Unless these changes are properly integrated, they might easily produce maladjustments in the internal economy of the animal. An internal environment suitable for temperatures near freezing might be wholly unsuitable

for body temperatures approaching 100° F. This phenomenon is emphasized in the marked differences in response to drugs at various body temperatures.[25] Since temperatures of such magnitude might be impressed within the course of a few hours on land-living cold-blooded animals, the development of a coordination center would provide survival value for its possessor. In cold-blooded animals, such a center, without capacity to regulate the body temperature, apparently operates to adjust some elements of the internal environment in coordination with the changes induced by the external environment. A rapidly reacting center of this kind appears to have developed in the hypothalamus of the cold-blooded animals, and the temperature-regulating apparatus was later laid down in close proximity to it.

The development of heat-conservation, heat-production, and heat-loss mechanisms introduced a new factor—the relative constancy of the body temperature. The actual body temperature levels of a given animal group appear to be related to body size (Fig. 5).[15] These relatively constant conditions served to mask the basic integrative equipment, and the internal adjustments become minimal during the diurnal cycle.

A number of patterns of behavior of warm-blooded animals suggest the retention of some of the elements of the daily temperature cycle of the cold-blooded animals. These include the diurnal variations in body temperature, blood pressure, heart rate, fluid and blood shifts, and the period of torpor (sleep).[26]

The phenomenon of hibernation in warm-blooded animals may be considered a remnant of the response to seasonal temperature changes, and in this way similar to hibernation in cold-blooded animals.[24] Several functions such as the reproductive cycle, which previously depended in part on the daily variations in temperature and their annual recurrence, as a result of the constant body temperature have escaped from the solar cycle and established a reproductive pattern independent of the sun. For example, in the female the estrus cycle is closely tied to body temperature variations which reflect the annual cycle. Prior to ovulation the body temperature is low; it then rises and remains high during the proliferative and/or the pregnant period.[27, 28] Curiously, certain species of plants will not flower unless the plant experiences a rapid drop in temperature.[29]

A most interesting problem of the constant high body temperatures of the warm-blooded animals is that of the peculiar association of the sexual cycle with body temperature and the thermal vul-

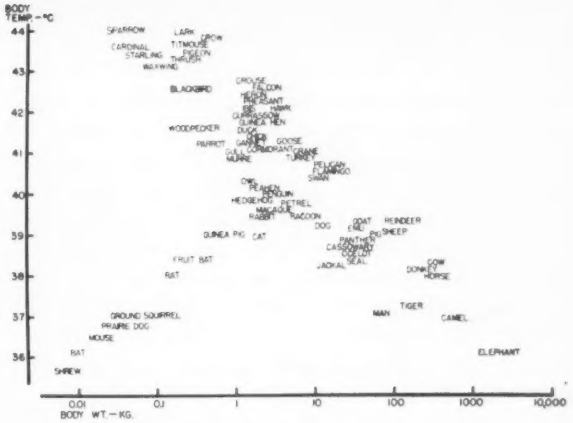


FIG. 5. The resting body temperatures of mammals and birds plotted against an average body weight. From SCIENCE, **111**, 465 (1950), with correction.

nerability of the spermatozoa. In many animals with high constant body temperatures the testicles are protected by their expulsion from the body proper into the air-cooled scrotum.[30] In birds, Cowles has suggested that the migration of the testicle upward, to a close approximation with the relatively cool air sacs of the lungs, represents a similar mechanism.[31] The diurnal cycle operates to permit spermatogenesis in some birds by means of the nocturnal drop in body temperature.[30]

Fever has commonly been interpreted as a mechanism aiding the body in overcoming some infective agent. However, only a few microbes such as the spirochete and the gonococcus are known to be injured during fevers. On the other hand, fevers occur in many conditions in which they confer no known advantage. It is well known that a rise in body temperature of only a few degrees may seriously disturb the normal function of the body[32] and thus weaken the ability to resist infection. In fact, an uncontrolled rise in body temperature may in itself lead to exhaustion and to death.

The great neurologist, Hughlings Jackson, long ago pointed out that the more recently developed a function of the nervous system may be, the more susceptible it is to injury.[33] For example, the slightest toxicity, such as that which results from the lack of oxygen, or from small quantities of alcohol, may seriously disturb one of our most recently developed functions, that of judgment. By the same token, if the heat-loss mechanism is of recent development (Fig. 4), it would be expected that it would be most susceptible to injury. Invasion of toxic materials into the blood stream would thus disturb the new and sensitive heat-loss mechanisms, while the older heat-conservation and heat-

production mechanisms remained relatively unaffected. The dysbalance would result in an elevation of the body temperature. Thus it would appear that fever is seldom laudable, operating as a defensive mechanism, but is usually nothing more than an indication of disturbed function. The common experience that infants and children may run high fevers as a result of relatively innocuous infections, testifies to the instability of the heat-loss mechanisms in the young.[34] In older individuals, as the mechanism becomes more stable, even severe infections may produce only minimal rises in body temperature.

The development of warm-bloodedness could not have taken place without the provision of an adequate supply of fuel and oxygen to maintain the fires of the body. The logistic problem was solved in the course of the long and involved evolution of the heart from the relatively simple pump of the fish, capable of developing only low pressures, to the complicated "double" heart capable of generating the much higher blood pressures required by the warm-blooded animals.[35] With these higher pressures, greater rates of blood flow can be obtained to supply the rapidly metabolizing tissues. These higher pressures also made it possible for animals to raise their heads skyward for the first time in history, since the pressure was now high enough to pump the blood upward against gravity. However, it also became possible for the pressure to be raised to such high levels that it might have deleterious effects on the blood vessels and even upon the heart itself.

The ancients tell us that Zeus was infuriated by the theft of the heavenly fire by Prometheus who gave it to man. To vent his spleen, Zeus fashioned the all-endowed Pandora, gave her a jar filled with evils, and sent her as a gift to Prometheus' brother, Epimetheus. She was accepted despite the dire warnings of Prometheus about gifts from the Greek gods. The opening of the jar and the escape of its contents was the inevitable outcome.[36] We may look in similar vein upon the mechanisms for adjusting to the Promethean fire of warm-bloodedness which has brought on us a Pandora's vessel containing high blood pressure, hardening of the arteries, and heart failure, but also Hope.

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Sets of Three Measurements

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SINGLE measurements have meaning only when they are made against a background of familiarity with the measurement procedure and the object that is measured. Without this background the numerical value obtained by making a single measurement is inherently misleading. The numerals set down to represent the measurement convey a sense of exactness to the unwary. Unless there exists some prior information about the agreement that might be expected between this measurement and a second measurement, the zone of uncertainty is so large that the first measurement, by itself, can hardly be said to convey any quantitative information. Certainly, no experienced person would be willing to hazard a guess as to how close such a single measurement represents the phenomenon or class of measurements of which this measurement is a single representative.

A second, truly independent, measurement does a good deal more than fortify the first measurement by making it possible to use the average of both measurements. The *difference* between the two measurements provides the information that is indispensable if limits about the average are to be designated that may, with some stated degree of certainty, include the unknown value for the class. The average of the two measurements is an estimate of the class value. Notice that the difference between the two measurements is a single estimate of the average difference. This estimate of the difference could be improved if more measurements were available for examination. The limits that are laid off above and below the average are very sensitive to the number of measurements available for estimating the errors in the measurements. Working at the 95 per cent level of certainty, the limits, if five measurements are available, are only 14 per cent as wide as those that must be set if only a single pair is taken. The measurements are assumed to be normally distributed.

The largest part of this reduction in the spacing of the limits comes, not through the greater stability of an average of five over an average of two, but by virtue of the much better information about the dispersion or errors in the measurements. The 95 per cent limits set around the average of two measurements when the dispersion is known exactly are only 15 per cent as wide as those that must be set if the single difference between the two measurements constitutes the sole information on the dispersion. In other words, an average based on two measurements, when the dispersion is known, is about as useful as an average of five measurements when these five measurements must also supply the information on the dispersion. To anticipate the objection that the dispersion is never known exactly, it is sufficient to remark that the above percentage is slightly increased to 16 if the "known" dispersion is based on as many as three score measurements.

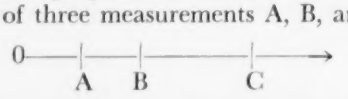
The preceding paragraphs stress the importance of knowledge regarding the errors in measurements. An experimental worker is naturally interested chiefly in the average, because this is the magnitude sought in connection with the scientific problem under study. The errors in the measurements are inevitably regarded as a nuisance. Every experimenter makes strenuous efforts to reduce experimental errors so that the zone of uncertainty is small. The point being made here is that the zone of uncertainty may also be reduced if attention is given to obtaining a good estimate of the errors.

Because experimenters have for so long been successful in reducing the errors in their measurements to the point that they could almost be ignored, they have tended to give very little study to the behavior of the errors of measurements. Extraordinary as it may seem, experimenters taking measurements every day are unaware of relatively simple properties of the sets of data they enter in their notebooks. Perhaps it is unnecessary to know much about the behavior of measurements in order to interpret the experimental results. More likely,

by reason of long experience, workers develop some sort of unstated working rules that are used more or less automatically in forming judgments. Nevertheless, the properties of measurements are themselves quite interesting.

Many believe that it is necessary to have rather large sets of measurements in order to make use of statistical techniques. This is not at all the case, as was shown in the opening remarks regarding pairs of measurements. Given a sequence of pairs, or triads of measurements, a great deal can be learned about the behavior of measurements by simple empirical procedures. It is not necessary that all the pairs or triads refer to the same object. The objects measured may be different. The only requirement is that the measurement procedure have the same errors of measurement over all of the sets. It is not uncommon to have as many as three measurements on the same object. Any one set tells very little; but if many sets are available, certain patterns of behavior are easy to establish by examination. The patterns may also be predicted by mathematical procedures. The study of these patterns is a major activity in mathematical statistics.

A simple question will illustrate what is meant by the phrase "the behavior of measurements." Consider a set of three measurements. It is hardly likely that these can be obtained simultaneously. They are, at least, put down in some time sequence. After the first two are entered, the experimenter may be curious about how often the third measurement falls between the two already entered. The question does not apply to any one set. The question is: In general, over all sets of three measurements, how often is the third measurement intermediate between the first and second? This is a good question because no assumption needs to be made that the measurements are normally distributed. One empirical way to find the proportion of triads in which the third measurement lies between the first two is to go down a long list, say 100, of sets of three measurements. Check the sets in which this event occurs and count up the checks. This is an answer, but certainly not an exact answer. Another set of 100 triads would very likely give a different count. The correct proportion can be approached as closely as desired by sufficiently increasing the number of sets examined. It is easier, this time, to use a simple bit of logic to go directly to the exact proportion. Indicate on a scale the positions of three measurements A, B, and C.



Obviously, B does lie between A and C. Now, these three measurements might have been obtained in any order. There are only six orders in which the measurements can be arranged.

Order	1	2	3
	A	B	C*
	A	C	B
	B	A	C
	B	C	A
	C	A	B
	C	B	A*

The asterisks show the two, out of the six equally likely orders, that bring B between A and C. And so the answer is that one-third of the sets of measurements will be such that the third measurements falls between those already in hand. Two times out of three, on the average, the third measurement will be smaller than both or larger than both the first pair. This proportion is a property of data; probably not a very useful property. It cannot be changed by anything the experimenter can do. But an easy generalization may be of interest. Suppose nine measurements have been made. Inevitably these are scattered over a range of values. Most experimenters would rather expect a tenth measurement, if made, to fall somewhere in the range of values already encountered. There is a formula to find how often this will occur. If $(n-1)$ measurements are followed by an nth measurement, the chance that this measurement falls between the smallest and largest of the $(n-1)$ measurements is $(n-2)/n$. Thus, once out of five times, the tenth measurement will be either smaller or larger than the other nine. Many might suspect a tenth measurement that fell outside the range of nine measurements. Once in five times is hardly a rare event and it would be unwise to suspect the tenth measurement on this ground.

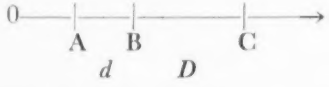
It is customary in school courses in quantitative chemical analysis to require students to turn in duplicate analytical results. The grade for the analytical work depends in part on how well the average of the two determinations agrees with the value ascribed to the material, and in part on how well the two determinations agree with each other. It is rumored that students have discovered that it takes practically no longer to run three determinations than it does to run two. The student looks at the three results and turns in the pair showing the best agreement. If this practice exists there should be some interest in the consequences. First, the average agreement between such a selected pair is very much better than the average difference found between two unselected measurements. The difference between the selected measurements averages four-tenths $(3-3\sqrt{3}/2)$ that of the differ-

ence for honest duplicates.[1] Consequently, this practice would lead a chemistry professor to form an altogether optimistic picture of the precision he can properly expect from students. It would be tough going for a student who just ran duplicates.

There is, however, something more to be said about this selection of the best two out of three. How about the behavior of the average of such a selected pair? The dispersion of this selected average is greater than the dispersion shown by the average of duplicates. In other words, the student may lose on that part of his grade. The operation makes the average behave as though the measurement errors were about 12 per cent larger than they actually are.

There are certainly many individuals who have been tempted to save the best two out of three measurements. Until very recently, no one, experimenter or statistician, had thought to explore the consequences of such selection. Most people are very much surprised to find how pronounced an effect there is on the apparent agreement of two measurements if they are selected from three. Misinformation on the real dispersion of measurements can be as misleading as tampering with the actual values.

The temptation to discard one of three measurements comes from the frequent occurrence of sets in which two measurements are in close agreement and the remaining measurement is considerably removed from the pair. Intuitively, one might argue that perhaps some blunder was made on this far removed value. Unless there is available a realistic picture of the ways three measurements may distribute themselves, when there is nothing whatsoever wrong with any of the measurements, any judgment as to whether or not a blunder has been committed is almost certain to err in discarding good values. Again, mark the measurements off on a linear scale, and denote the distances between the measurements by d and D. It has been shown that



the interval D is ten or more times as large as d in 15.7 per cent of sets of three measurements. Rather more than one-third of the time, D is at least four times as large as d. Evidently, very uneven spacing of the measurements is a commonplace. Values of the ratio D/d exceed 32.57 once in twenty times.[2] If other knowledge about the dispersion is lacking, there is scant justification for discarding the remote value unless it is removed from the closest pair by an amount some thirty times the difference between the measurements constituting the closest pair.

Given three measurements, there is always a smallest one. Ordinarily, in order to find out which is the smallest measurement, it is necessary to have all three available. There are circumstances that reveal the smallest measurement directly. There is a tensile test for steel alloys used in turbo-engines that specifies that test specimens be maintained at a constant temperature in the neighborhood of operating temperatures and subjected to a constant load. The time to rupture is recorded. Often this test extends over weeks. Imagine that three test specimens are mounted in tandem and the load attached to the bottom specimen. Eventually, one of the specimens yields—the one with the smallest strength. It is not necessary to continue the test to determine the times for the remaining two specimens. Now, suppose that this test on three specimens is repeated over and over and the results for all the least of threes examined. They will be found to have much less dispersion among themselves than the results of tests with single specimens. Indeed, the dispersion of these smallest values is about that shown by the averages of two single tests. Apart from the bias below the average, each least of three is nearly equal to the average of two ordinary measurements as far as dispersion is concerned. The bias can be allowed for if the dispersion is known or estimated from a reasonable amount of data. Besides the diminished dispersion, there is the further advantage that the average time for the least of three is shorter than the time for a single specimen (by the amount of the bias), and thus more experiments may be run. The whole subject of extreme values has received much attention in the past few years.

Sets of three measurements sometimes arise even when the usual schedule calls for taking duplicates. Frequently the chief function of the second measurement is to reassure the experimenter that no gross blunder was made on the first measurement. Assuming that a familiar measuring procedure is being used, the experimenter will have in mind the average agreement between duplicates for this procedure. He will need to set up some operating rule to warn him that one of the measurements may be subject to an error beyond that usually associated with his procedure. It is known that five per cent of the pairs will, in the normal course of events, turn up with a difference between the measurements that is 2.45 times as large as the expected difference between members of a pair.[3] It may seem desirable to take some action in such cases lest this large difference really arise from some undetected

slip. This rule will catch slips at an insurance premium of five per cent of the work needlessly held for further examination.

There is no way, merely by looking at widely separated results, to tell which one of the two measurements is really at fault, if a slip has been committed. It would be a tremendous boon to scientists if, simply by looking at the results, they could always pick that measurement which is actually nearer to the true average of the class of measurements to which these two belong. If this measurement could be singled out, it would be best to throw away the other measurement because this chosen measurement has just about the dispersion that averages of three measurements show!* Lacking any convenient crystal ball, the experimenter must have recourse to taking at least one more measurement. In this way a set of three measurements is generated and the experimenter looks at these to decide what to do next. Apparently there is no generally accepted set of rules to guide the experimenter in this situation. Fortunately, if a serious blunder has been made on one of the original pair, the third measurement usually pairs closely with one of the first two and the remaining measurement is clearly so far away that it can be discarded without hesitation.

The experimenter is still left with about five per cent of his sets which appear not to have a serious blunder. Shall he continue to use the rule of selecting the closest pair? Notice that this is not the same closest pair discussed earlier. It is the closest pair subject to the requirement that the *third* measurement be one of the pair. Examination of these sets of three will reveal that, for about half of them, the over-all range for all three measurements is not unreasonable for sets of three. Ninety-five per cent of sets of three have an over-all range not exceeding 2.96 times the average difference for a pair. Sets that meet this requirement may be considered to have been freed from suspicion and the average of all three results may be taken at best representing the quantity sought. The remaining sets of three have a suspiciously large range, and it appears reasonable to discard that one of the first two which is chiefly responsible for the large range. It is assumed that the third measurement turns out not to differ too much from the closer one of the first pair. Experimenters will be inclined, therefore, to take the average of the third measurement and the one closer to it. There is a surprising property about the averages of these pairs. These averages appear to have just about the same dispersion as the single third measurement. Exact statistical theory is not yet available on this point. The experimenter may either accept the third measurement (discarding the first pair) or take the average of the third and the closer one to it. The latter procedure is certainly the better choice when a blunder has been made; so it would appear that it is simpler always to take the average of the closest pair, one of the pair being the third measurement.

Just recently, a statistical solution has been found for one more problem involving sets of three.[4] The problem is particularly interesting because the set of three, instead of referring to three measurements on one object, has to do with measurements on three different objects. The purpose here is to be able to pick out, with any desired frequency of success, the object having the largest value of the property being measured. A little reflection suggests that the frequency of success will depend upon how much larger than the other two the largest object really is. Success will also depend upon the dispersion of the measurements and the number of measurements made on each object. For simplicity, assume that just one measurement is made on each object. What are the conditions so that, 95 per cent of the time, the largest measurement recorded will actually come from the largest object? The largest object must in fact differ from the nearer of the smaller two by an amount equal to 2.40 times the average difference betwen two measurements on the same object. (The dispersion of the measurements is assumed to be known.) For the first time, a firm guide exists to a long prevailing problem of picking with high assurance the best catalyst, the best tire, or any other entity. A simple rule leads directly to the number of measurements that must be made on *each* of the three objects, if it is seen in advance that it is desirable to succeed, even if the difference important to detect is less than the above multiple of 2.40 times the average difference for pairs of measurements on the same object. To cut the multiple to 1.20, that is one-half, will require four measurements on each object. To cut it to 0.80, or one-third, will require nine. To cut the factor to unity, or the same magnitude as the average difference, takes six measurements on each.

Perhaps the more important point about the above problem is that the experimenter can, and must, do his statistical thinking in advance of approaching the three objects with his measuring equipment. In effect, he has to decide what magnitude of difference he would regret failing to detect, and he must know his equipment and the rule

* My colleague, E. P. King, derived for me the variance of the less erroneous one of two measurements. It is $1 - 2/\pi$.

He knows then that, *if* one of the objects differs from the others by at least this amount, his largest measurement (or average) will point to it at least 95 per cent of the time. This is not at all to say, after the measurements have been taken, that one is 95 per cent sure that the largest measurement came from the largest object. Obviously if the objects are all equal, or very closely so, any object has about equal chance (one-third) of yielding the largest measurement. The experimenter must remember that he has already decided that, if the objects are that close, he does not care which one he picks. He does know that he is reasonably sure that he has picked the largest object *if* it differs appreciably from the others.

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SCIENTIFIC RESEARCH—1915*

As members of the body politic, we can assist the development of science in two ways. *Firstly,* by doing each our individual part towards ensuring that endowment for the university must provide not only for "teaching adolescents the rudiments of Greek and Latin" and erecting imposing buildings, but also for the furtherance of scientific research. The public readily appreciates a great educational mill for the manufacture of mediocre learning, and it always appreciates a showy building, but it is slow to realize that that which urgently and at all times needs endowment is experimental research.

Secondly, it is vital that public sentiment should be educated to the point of providing the legal machinery whereby some proportion, no matter how small, of the wealth which science pours into the lap of the community, shall return automatically to the support and expansion of scientific research. The collection of a tax upon the profits accruing from inventions (which are all ultimately if indirectly results of scientific advances) and the devotion of the proceeds from this tax to the furtherance of research would not only be a policy of wisdom in the most material sense, but it would also be a policy of bare justice.——The author, born in Edinburgh, attended the University of Adelaide and from 1905 to 1916 he was a member of the faculty of the University of California. In 1920 he became Professor of Biochemistry and General Physiology at the University of Adelaide. He was the author of numerous technical articles and books.

* From *The Cash Value of Scientific Research,* by T. Brailsford Robertson, The Scientific Monthly, **1,** 140 (1915).

Time-Binding and the Concept of Culture*

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IN June, 1921 an important book was published by an unknown author. The book was entitled *Manhood of Humanity*, and its author was Alfred Korzybski. By December, 1921 it had gone into its third printing, and in December, 1923 there was a fourth printing. Obviously, quite a number of people had found the book of interest, but who they were I cannot tell. Certainly, it seems, the book did not make much rumor in the world or leave too deep an impression. I read this book for the first time in 1950 in its second posthumously published edition of June, 1950. It was only then that I learned that Korzybski's theory of time-binding had been developed long before the appearance of *Science and Sanity* in 1933. It is to be hoped that with the republication of *Manhood of Humanity* the book will, at last, come into its own, for it is perhaps more timely for our own day than it appears to have been for the time during which it made its first appearance. For in *Manhood of Humanity* Korzybski attempted to lay the foundations for the science and art of what he then called by the new term Human Engineering—a term which has since been adopted by others, but not quite in the sense in which he meant it. Such a science, Korzybski pointed out, must be based on a true understanding of the nature of man, upon what is essentially characteristic of him; and he endeavored to show that what distinguishes man from all other living things is his capacity for time-binding.

Before proceeding to the discussion of this brilliant conception I must say a few words about the dangers of making one's appearance too early. Intellectually, Korzybski was a prematurely born child. The views set out in *Manhood of Humanity* were from 25 to 30 years too early. While in 1921 Korzybski was occupied with science and its relation to ethics, with science and its relation to values, his contemporaries in the sciences and in philosophy were denying that science or philosophy could provide a sound scientific foundation for leading the good life. We find Bertrand Russell, for example, writing, and Korzybski quotes this very passage, "The hope of satisfaction to our more human desires, the hope of demonstrating that the world has this or that ethical characteristic, is not one which, so far as I can see, philosophy can do anything whatever to satisfy." With this viewpoint Korzybski was entirely unable to agree, and I think, if we are to judge from his latest writings, Russell would today be more inclined to agree with Korzybski's 1921 judgment than he would with his own of earlier years.[1] Said Korzybski, in 1921, "The scientists, all of them, have their duties no doubt, but they do not fully use their education if they do not try to broaden their sense of responsibility toward all mankind instead of closing themselves up in a narrow specialization where they find their pleasure. Neither engineers nor other scientific men have any right to prefer their own personal peace to the happiness of mankind; their place and their duty are in the front line of struggling humanity, not in the unperturbed ranks of those who keep themselves aloof from life. If they are indifferent, or discouraged because they feel or think that they know that the situation is hopeless, it may be proved that undue pessimism is as dangerous a 'religion' as any other blind creed."[2]

This was a view which was not popular when

* Based on a paper presented at the first annual series of the Alfred Korzybski Memorial Lectures held at the Waldorf-Astoria Hotel, New York, on March 5, 1952, under the auspices of the Institute of General Semantics, Lakeville, Connecticut.

Korzybski wrote. Scientists were inclined to an ivory-towerism and a hand-washing indifference to the consequences of their work. Thirty years later scientists are quite generally agreed that they must take a more responsible view of their place in the world. Korzybski's warning has come home to roost, "If those who know why and how neglect to act," he wrote in 1921, "those who do not know will act, and the world will continue to flounder."[3]

Korzybski was among the first to point out that "it is the great *disparity* between the rapid progress of the natural and technological sciences on the one hand and the slow progress of the metaphysical, so-called social 'sciences' on the other hand, that sooner or later so disturbs the equilibrium of human affairs as to result periodically in those social cataclysms which we call insurrections, revolutions and wars."[4] In the history of the development of the consciousness of this fact Korzybski's name will come to occupy an honored place. When a society places its emphases on technology it produces a nation of technicians. Korzybski saw quite clearly where the emphasis must be placed if mankind is to survive—namely, upon the understanding of man's nature and the development of human relations on the basis of that understanding. The good life was what Korzybski was passionately interested in, and so he said, "Ethics is too fundamentally important a factor in civilization to depend upon a theological or a legal excuse; ethics must conform to the *natural* laws of human *nature*."[5] Korzybski clearly stated the consequences of "A system of social and economic order built exclusively on selfishness, greed, 'survival of the fittest,' and ruthless competition,"[6] and prescribed the remedy. The period of the *childhood* of humanity was one of arbitrary thought and confusion. "The period of humanity's *manhood* will," Korzybski wrote, "I doubt not, be a scientific period—a period that will witness the gradual extension of scientific method to all the interests of mankind —a period in which man will discover the essential nature of man and establish, at length, the science and art of directing human energies and human capacities to the advancement of human weal in accordance with the laws of human nature."[7]

Korzybski died on March 1, 1950. Happily, he lived to see his best hopes for the application of the scientific method to some of the interests of mankind get started, even though fitfully. The dangers of being born too early are largely personal. One suffers the pains of inappreciation for being ahead of one's time, and when one is caught up with by the rest of mankind one has to have sense of humor enough to bear with equanimity the comment that after all there was nothing new in one's ideas, or that they were known all the time. Those who knew him can testify to the fact that Korzybski had a delightful sense of humor, so that neither the barking of the dogs of St. Ernulphus nor the patronizing gestures of the world of pernicious academia too much disturbed him.

In the present essay I wish to consider one of Korzybski's most significant contributions toward the better understanding of the nature of man, namely, his conception of time-binding. Korzybski distinguished between the classes of life in the following manner: Since plants capture one kind of energy, convert it into another, and store it up, he defined the plant class of life as the chemistry-binding class of life. Since animals are characterized by the freedom and faculty to move about in space he defined animals as the space-binding class of life.

"And now what shall we say of *human* beings?" asks Korzybski, "What is to be our definition of Man? Like the animals, human beings do indeed possess the *space-binding* capacity but, over and above that, human beings possess a most remarkable capacity which is entirely peculiar to them—I mean the capacity to summarize, digest and appropriate the labors and experiences of the past; I mean the capacity to use the fruits of past labors and experiences as intellectual or spiritual capital for the developments in the present; I mean the capacity to employ as instruments of increasing power the accumulated achievements of the all-precious lives of the past generations spent in trial and error, trial and success; I mean the capacity of human beings to conduct their lives in the ever increasing light of inherited wisdom; I mean the capacity in virtue of which man is at once the heritor of the by-gone ages and the trustee of posterity. And because humanity is just this magnificent natural agency by which the past lives in the present and the present for the future, I define HUMANITY, in the universal tongue of mathematics and mechanics, to be the TIME-BINDING CLASS OF LIFE."[8]

Very properly, Korzybski emphasized the importance of grasping the meaning of this definition of humanity as a starting-point for discovering the natural laws of human nature—of the human class of life. Korzybski pointed out the immeasurable evils which have resulted from regarding man as a mere space-binder, an animal. In other words, Korzybski was from the first aware of the dangers of "biologism" or what I have elsewhere called "the pathetic fallacy," the belief that man is nothing

but a function of his genes—a fallacy which many contemporary thinkers have even yet not succeeded in avoiding. Korzybski recognized that man was characterized by "properties of higher dimensionality"; that everything which is really time-binding is in the human dimension.

Now, Korzybski's definition of humanity, of man, is virtually identical with the present-day anthropologist's definition of culture. Here are some definitions of culture taken from the latest anthropological works by leading anthropologists. Firth writes, "If society is taken to be an aggregate of social relations, then culture is the content of those relations. Society emphasizes the human component, the aggregate of people and the relations between them. Culture emphasizes the component of accumulated resources, immaterial as well as material, which the people inherit, employ, transmute, add to, and transmit. Having substance, if in part only ideational, this component acts as a regulator to action. From the behavioral aspect, culture is all learned behavior which has been socially acquired. It includes the residual effects of social action. It is necessarily also an incentive to action."[9]

Kluckhohn writes, "By 'culture' anthropology means the total life way of a people, the social legacy the individual acquires from his group. Or culture can be regarded as that part of the environment that is the creation of man. . . . The general abstract notion serves to remind us that we cannot explain acts solely in terms of the biological properties of the people concerned, past experience, and the immediate situation. The past experience of other men in the form of culture enters into almost every event."[10]

Herskovits writes, "*Culture is the man-made part of the environment.* Implicit in this is the recognition that man's life is lived in a dual setting, the natural habitat and his social 'environment.' The definition also implies that culture is more than a biological phenomenon. It includes all the elements in man's mature endowment that he has acquired from his group by conscious learning or, on a somewhat different level, by a conditioning process—techniques of various kinds, social and other institutions, beliefs, and patterned modes of conduct."[11]

White writes, "The physical category is composed of non-living phenomena or systems; the biological, of living organisms. The cultural category, or order, of phenomena is made up of events that are dependent upon a faculty peculiar to the human species, namely, the ability to use symbols. These events are the ideas, beliefs, languages, tools, utensils, customs, sentiments, and institutions that make up the civilization—or *culture*, to use the anthropological term—of any people, regardless of time, place, or degree of development. Culture is passed down from one generation to another, or, it may be borrowed freely by one tribe from another. Its elements interact with one another in accordance with principles of their own. Culture thus constitutes a supra-biological, or extra-somatic, class of events, a process *sui generis*."[12]

Compare with these definitions Korzybski's definition of culture or civilization in the 1921 *Manhood of Humanity*. Korzybski writes, "Civilization as a process is the process of binding time; progress is made by the fact that each generation adds to the material and spiritual wealth which it inherits. Past achievements—the fruit of bygone time—thus live in the present, are augmented in the present, and transmitted to the future; the process goes on; time, the essential element, is so involved that, though it increases arithmetically, its fruit, civilization, advances, geometrically."[13]

The faculty peculiar to the human species, namely, the ability to make complex use of symbols, to which White refers, and upon which the development of the time-binding or cultural capacity of man is dependent was, of course, fully grasped by Korzybski, and here, too, Korzybski was a forerunner of later thinkers in this field. Chapter IV of *Science and Sanity* is devoted to symbolism, and its very first words are "The affairs of men are conducted by our own, man-made rules and according to man-made theories. Man's achievements rest upon the use of symbols. For this reason we must consider ourselves as a symbolic, semantic class of life, and those who rule the symbols, rule us."[14]

This viewpoint had already been worked out by the distinguished philosopher Ernst Cassirer in his *Philosophie der symbolischen Formen*, published in three volumes in 1923–1929,[15] and in part made available in a new English version in *An Essay on Man*, published in 1944.[16] In the latter work, Cassirer writes, "Man has, as it were, discovered a new method of adapting himself to his environment. Between the receptor system and the effector system, which are to be found in all animal species, we find in man a third link which we may describe as the *symbolic system*. This new acquisition transforms the whole of human life. As compared with the other animals man lives not merely in a broader reality; he lives, so to speak, in a new *dimension* of reality. . . . Reason is a very inadequate term with which to comprehend the forms of man's cultural life in all their richness and variety. But all these forms are symbolic forms. Hence, instead of defining man as an *animal rationale*, we should define him as an *animal symbolicum*. By so doing we

can designate his specific difference, and we can understand the new way open to man—the way to civilization."[17] Writing at about the same time, and quite unbeknownst to each other, Korzybski and Cassirer had arrived at the same conclusions. Writing in 1944, Cassirer appears to have been quite unaware of Korzybski's work.

Sufficient, I hope, has been said to show that Korzybski's conception of time-binding and the anthropological conception of culture are virtually identical in character. It is rather sad to reflect that almost all anthropologists have overlooked this fact. It is to be hoped that this oversight will soon be remedied. Meanwhile, let us proceed to discuss certain consequences which follow from the general theory of time-binding or culture for the development of a science of human nature and the science and art of human relations.

Korzybski pointed out that "Human nature, this time-binding power, not only has the peculiar capacity for perpetual progress, but it has, over and above all animal propensities, certain qualities constituting it a distinctive dimension or type of life. Not only our whole collective life proves a love for higher ideals, but even our dead *give* us the rich heritage, material and spiritual, of all their toils. There is nothing mystical about it; to call SUCH a class a *naturally* selfish class is not only nonsensical but monstrous."[18] In this passage Korzybski comes very close to the discoveries which are only at this moment in process of being made concerning the nature of human nature. How right Korzybski was in referring to the belief that humanity is naturally selfish as nonsensical and monstrous. The belief in the inherent selfishness of man, his innate naughtiness, inborn evil, aggressiveness and hostility, has taken many forms, and in each of its forms it has done untold personal and social damage. In the first place, that belief has conditioned our attitude towards our fellow men, and not only to our fellow men, but to those utterly defenseless and dependent potential time-binders, the classes of babies, infants, children, and adolescents. Our belief in the inherent naughtiness of man has caused us to make out of the process of child-rearing a discipline of restraints and frustrations, which has had the effect of seriously crippling and distorting most human beings who have been exposed to it. There is rather good reason to believe that most of the personal and social tragedies which mankind has created and suffered from have been due to this erroneous belief and the consequences flowing from it as a result of the child-rearing processes it has conditioned. The belief in the innate naughtiness of man is very old, but in the nineteenth century it accrued unto itself a strong reinforcement in the doctrine of "the survival of the fittest." "The struggle for survival," "Nature red in tooth and claw," "dog eat dog," and similar phrases were used to describe the condition of animals in a state of nature. Since man is an animal, it was argued, he has inherited the same fundamental drives that keep the rest of the animal kingdom going. Hence, man is by nature aggressive and competitive, and, it was asked, do we not see this in the evolution of human societies, and in the struggles of the classes in society? The Spencerian application of the theories of Darwin to the struggle for survival in human societies, known as Social Darwinism, further lent enchantment to this particular view of the relations of men to one another.

Alfred Korzybski (Photo courtesy K. S. Keyes, Jr.)

Searching for the inherent naughtiness and aggressiveness of the infant at birth and thereafter, contemporary investigators have been unsuccessful in finding any evidences of it. The distinguished New York child psychiatrist, Dr. Lauretta Bender, having handled thousands of children, has written from her great experience, in an important article entitled "The Genesis of Hostility in Children," that far from being inborn, hostility or aggression in the child "is a symptom complex resulting from

deprivations which are caused by developmental discrepancies in the total personality structure such that the constructive patterned drives for action in the child find inadequate means of satisfaction and result in amplification or disorganization of the drives into hostile or destructive aggression." "The child," she writes, "acts as though there were an inherent awareness of his needs and there is thus the expectation of having them met. A failure in this regard is a deprivation and leads to frustration and a reactive aggressive response." Indeed, the creativeness of the organism is directed toward maturation in terms of cooperation. Bender calls it "the inherent capacity or drive for normality." And, as she says, "The emphasis on the inborn or instinctive features of hostility, aggression, death wishes, and the negative emotional experiences represents a one-sided approach which has led our students of child psychology astray."[19]

Maslow, in an article entitled "Our Maligned Animal Nature," writes, "I find children, up to the time they are spoiled and flattened out by the culture, nicer, better, more attractive human beings than their elders, even though they are of course more 'primitive' than their elders. The 'taming and transforming' that they undergo seem to hurt rather than help. It was not for nothing that a famous psychologist once defined adults as 'deteriorated children.'" "Could it be possible," Maslow inquires, "that what we need is a little *more* primitiveness and a little less *taming*?"[20]

Babies are born cooperative. What they want is to be cooperated with and to cooperate. When their needs for cooperation, for love, are frustrated, they may react with aggressive behavior. Aggressive behavior is originally a means of seeking and if possible compelling love. Aggression is practically always, if not always, the effect of love frustrated, or of the expectation of love frustrated. Children are not born selfish, they are made selfish by being forced to attend to their own needs as best they can by the failure of their discipliners to attend properly to their needs for cooperation. The natural selfishness of the child is, indeed, a monstrous notion. It is an unfortunate projection of themselves upon the child of those who hold it. Man is not born evil, nor is he born neither good nor evil, but in a very positive sense he is born good. Good in the sense of conferring survival benefits upon all with whom he comes into social relations. When one analyzes the basic needs of the human organism, those needs which must be satisfied if the organism is to survive, one finds that they are oriented in the direction of cooperation, of wanting to love, as well as wanting to be cooperated with and loved. It is in this sense that the organism may be said to be born good, and it is one of the few senses in which the word "good" means anything.[21]

Even if man had inherited any drives toward aggressiveness and combat, by virtue of his capacity for time-binding he would be capable of controlling the expression of such drives so as to negate and completely nullify their potencies. As for the doctrine of "the survival of the fittest," Korzybski pointed out that this "in the commonly used animal sense is not a theory or principle for a 'time-binding' being . . . its effect upon humanity is sinister and degrading."[22]

Thomas Henry Huxley, in a letter written October 27, 1890, wrote, "The unlucky substitution of 'survival of the fittest' for 'natural selection' has done much harm in consequence of the ambiguity of the 'fittest'—which many take to mean 'best' or 'highest'—whereas natural selection may work towards degradation *vide epizoa*."[23]

What, indeed, is "fitness"? The Social Darwinists overlooked the fact that fitness is related to the current environment of a group, and without further scruple they converted "fittest" into "best." The current environment of man, perhaps more than at any time during his whole history, demands that he realize pretty rapidly what his fitness must consist in if he is to survive. As Korzybski pointed out in 1921, "There is indeed a fine sense in which we can, if we choose, apply the expression—survival of the fittest—to the activity of the time-binding energies of man. Having the peculiar capacity to survive in our deeds, we have an inclination to use it and we survive in the deeds of our creation; and so there is brought about the 'survival in time' of higher and higher ideals. . . . It must be emphasized that the development of the higher ideals is due to the *natural* capacity of humanity; the impulse is simply time-binding impulse."[24]

Fitness for man, perhaps more than for any other creature, has fundamentally always consisted in and must increasingly come to consist in the subordination of individual competition to interpersonal cooperation, and of intergroup competition to cooperative association.* For this purpose it will be necessary to follow the highest ideals. But what are the highest ideals? Are they the same for all mankind? To answer the second question first: The ideals of human fitness are, indeed, the

* This is a précis of the words written by Patrick Geddes and Arthur Thomson in their book *The Evolution of Sex*, London: Scott, p. 311, (1889). "Each of the greater steps in progress is in fact associated with an increased measure of subordination of individual competition to reproductive or social ends, and of interspecific competition to cooperative association."

same for all mankind because all mankind is human and because these ideals are determined by the nature of human nature itself. As Korzybski stated, "Human logic— . . . the logic *natural* for man—will show us that 'good' and 'just' and 'right' are to have their significance defined and understood entirely in terms of human nature. Human nature—not animal nature—is to be the basis and guide of Human Engineering."[25]

Korzybski's contribution to the understanding of human nature did not lie in the fine analysis of its structure, his contribution lay in his distinguishing man from all other living creatures as the time-binder, and to emphasize the fact that this capacity is a natural one. Unlike Thomas Henry Huxley, however, he saw that man does not have to struggle against his inner nature but, rather, that he must realize it; for human nature, innate nature, was good, *not* evil. And Korzybski held that the discovery of the highest ideals should be based on the scientific understanding of the nature of human nature, in other words, that "what is right for man is what is right for human nature." I believe that when everything is said and done this assertion will be found to represent the fundamental formula for the life of man; that what is right for man is what is right for human nature. To some of us who are working in this field it is already evident that this is, indeed, so. For the findings of modern workers have shown unequivocally that in so far as men depart from the biologic demands of their innate nature they fall ill and become disoperative, and that in so far as they conform to the requirements of their nature they function harmoniously and well. But in order to recognize this fact it is first necessary to understand the nature of human nature. This is an area in which the most significant work has been done in the twentieth century, and in which a tremendous amount of work remains to be done before we can really speak of understanding the nature of human nature; but for some who have been interested in putting together the findings of modern science on the nature of human nature the general picture is clear, and it is in complete conformity with Korzybski's views. I now propose to give an outline of that general picture.

Man is, of course, part of the world of Nature. He comes into being in fundamentally the same way as all other creatures, that is, by reproduction. The reproductive process is the fundamentally social process, and it is this reproductive process which determines the pattern of the biologic life of man, and of all other living creatures as well. The process of reproduction is an interdependent dependent one, and that is the essence of the biologic relation between organisms, interdependency and dependency. This holds true whether we are concerned with unicellular or multicellular organisms, for the amoeba or for man. Man, however, is perhaps more strikingly characterized by this interdependency-dependency trait than any other living creature. Following conception he spends nine months in that dependent-interdependent state, and after birth he continues to spend several years in dependent-interdependent relationship of the most dependent kind with other human beings. Dependency is man's basic biologic state and interdependency is man's basic social state. Man comes into the world biologically wanting to be dependent and interdependent. How do we know this for a fact? We know this for a fact because if the interdependency needs of the potentially human organism are not satisfied the organism simply does not develop as a human being, and if its dependency needs are not satisfied it can develop only in a crippled sort of way. The infant's drives are oriented in the direction of love and cooperation, it wants to love and it wants to be cooperated with, and if it is loved and cooperated with it then develops in every way more efficiently as a loving cooperative human being, and also as a healthy physical organism. How do we know these things for facts? We know these things for facts because if the potentially human organism is not adequately loved or cooperated with it fails to develop as an adequate human being, as a being who is functioning harmoniously and who is adequately capable of loving and cooperating with others. The evidence for these relationships has recently been ably discussed and summarized in a World Health Organization publication entitled "Maternal Care and Infant Health" and written by the English psychiatrist John Bowlby.* As for the relation between adequate love in infancy and proper physical growth and development, the work of Dr. Ralph Fried at Cleveland[26] and of others elsewhere is conclusive. This should not surprise us in the least, for the organism is a whole, and one cannot deprive it of necessary stimulations in any part of it without affecting the whole organism—that is what the word "organism," properly understood, in part implies. Not only this, but we now know that when children within the first year and during the greater part of their first six years are adequately loved they grow up to be adequately loving and cooperative healthy persons, persons who are capa-

* Issued in the United States by Columbia University Press, New York, 1951.

ble of taking the stresses and strains of life very much more efficiently than those who have not been adequately loved during the first half dozen years.[27] From the studies which have been made among the non-literate peoples of the world, the so-called "primitive peoples," anthropologists have found that the way in which children are brought up is closely related to their personality structure; and on the whole, the conclusion is that the more adequately children have been loved during their first six years the more loving they are as adults, whereas the more frustrated children have been during their first six years the more frustrated they are as adult personalities and the more unloving. This may be putting the essence of the findings in an oversimplified form, but I do not know of any better way of stating the facts in a few words than in this manner.

What, in brief, has been discovered is that to live as if to live and love were one is the only way of life for human beings, because, indeed, this is the way of life which the innate nature of man demands. The highest ideals of man, therefore, spring from his own nature, and the highest of these ideals and the one which must inform all others is *love*. This is not a new discovery in the world; what is new is that scientists should have made it by scientific means. What contemporary scientists working in this field have done is to give, without in most cases being aware of it or intending to, a scientific validation to the Sermon on the Mount: To love thy neighbor as thyself: To do unto others as you would have them do unto you; principles which were enunciated by many philosophers, prophets, and seers, long before the birth of Christ. Indeed, every people has at sometime or another arrived at this elementary piece of wisdom on the basis of experience, and most peoples have more or less successfully attempted to live by these principles because they have discovered them to be the most efficient to live by. This, at any rate, is their theory enshrined in their religious and civil codes, but the practice seems to lag far behind the religious theory and the civil codes so frequently derived from and based on those religious theories. What is the reason for this difference between theory and practice?

In the light of our modern studies the answer to this question would not seem to be a very difficult one, although any answer that is returned is likely to be an oversimplification. The reason why people do not live by the principle of love is that they have not been raised by it. On the other hand, most of them have been raised by the principle of systematic frustration—which in our culture we often call "discipline." Most of the so-called civilized world simply has not loved little children adequately enough, no matter what their holy books have said about love, and if you have been pushed around as a child you are likely to grow up as a pusher-around of others when you are an adult. You can be terribly interested in love if you have been deprived of it as a child, but you will be unable adequately to receive it and unable adequately to give it. One may tell children that one loves them at the same time that one is frustrating them. What they remember is the frustration, not the "love." For what children believe in the behavior of others is what they do, *not* what they say.

How, then, is one to escape from this vicious circle: Frustrated children growing up into frustrating parents who in turn produce frustrated children who grow up to be frustrating parents? The answer is: By educating the world of human beings in the facts, by showing all who are capable of learning what the true nature of human nature is, and why it must be respected; what it is that human nature demands and why those demands must be obeyed, for as Bacon put it, "Nature to be commanded must be obeyed." We must teach all who are capable of learning—and everyone is capable of learning—what happens when you do not obey the innate demands of one's being, and what happens when one does. We must, in short, teach the art and science of human nature, the art and science of human relations. We must relieve mankind of the load of myths which has been weighing it down for so long about the nature of nature and the nature of human nature. We must disabuse mankind of the myth of its inherent naughtiness, and present the facts which will permit men to judge for themselves what the truth really is. But above all else we must teach men how to love. This can all be achieved by many different approaches and at innumerable different levels, but the one method in which I place the greatest faith is education. Such education must be conceived as the bringing out of the best that is within the person by making available to him all the encouragements and supports and stimulations which he requires to enable him to become a loving, cooperative, non-conflictful person; one who is not only aware of what is right with the world but also what is wrong with it, and who is equipped with both the knowledge and the desire necessary to improve it nearer that ideal of what it should and can be. Such a person will not be a competitor, but a cooperator, a person for whom altruism will

be a passion and selfishness a disorder; a person wise enough to know that

> He who would love his fellow men
> Must not expect too much of them;

a person who will want to improve the world as he finds it, and not accept things as they are, but who will also have the wisdom to know what things to accept and what to change; a person who will not risk wrecking the social machinery by exceeding the speed-limit of rational inquiry; who will not abolish anything but merely make it necessary to discontinue it, dispelling fear by supplying facts and knowledge; who will recognize the strange necessity of beauty; who will have a sense of personal responsibility for decency and justice; who will never burn the smoke of incense before an empty shrine; a person, in short, who, having had a loving order made within himself, will make loving order in the world.

> —be to other souls
> The cup of strength in some great agony,
> Enkindle generous ardour, feed pure love,
> Beget the smiles that have no cruelty—
> Be the sweet presence of a good diffused,
> And in diffusion ever more intense.
>
> GEORGE ELIOT

I conclude with some words from Korzybski's *Manhood of Humanity*:

"In humanity's manhood, patriotism—the love of country—will not perish—far from it—it will grow to embrace the world, for your country and mine will be the world. Your 'state' and mine will be the Human State—a Cooperative Commonwealth of Man—a democracy in fact and not merely in name. It will be a natural organic embodiment of the civilizing energies—the wealth-producing energies—characteristic of the human class of life. Its larger affairs will be guided by the science and art of Human Engineering—not by ignorant and grafting 'politicians'—but by scientific men, by honest men who *know*.

"Is it a *dream*? It is a dream and science will make it a reality."[28]

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Psychology as a Science

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THE approach I would like to take to this discussion of psychology as a science is simply that, whether or not such is generally the case now, it is possible to approach the problems of psychology scientifically. Furthermore, it seems to me that about our only hope in this task lies in making use of the things that the philosophers of science are able to tell us. I have, therefore, tried to decide just what lessons these philosophers can teach us and how they can be applied to psychology. My sincere hope is that the summary of my thinking on the topic which I am about to present does not do the philosophers too great an injustice, or, if it does, that they can at least be philosophical about it.

Let us begin by stating the subject matter with which psychology deals. The standard definition of psychology is that it is *the science of behavior*. This definition probably needs clarification. On the one hand it covers less than you may have expected. On the other, it covers too much. Note first that the definition says nothing about mind, or experience, or consciousness. This is not merely an attempt to finesse the problems raised by these three terms. If they are to be used at all in a science of psychology, they will have to be introduced in a way to be described later. A second point is that the behavior with which the psychologist deals tends to be limited to its grosser aspects. Where internal physiological processes are a part of psychology, they are conceived as aids to conceptualization rather than as a part of its basic subject matter. I shall return more than once to the question of the relationship between psychology and physiology; let us turn now to a discussion of the use of the term, "science" in the expression, "science of behavior."

The word, science, refers to the attempt that people called scientists make to bring order into the world of observable events. Scientists are, of course, not the only ones trying to bring order into the world. Others are the theologians, some literary people, and the philosophers. The scientific way of approaching the task differs from these latter ways, chiefly in terms of its initial data. All science begins with the sensory experience of a perceiving scientist. These sensory experiences, which are private and not a part of science, give rise to its initial data, that is, to a publically observable *report* of such experiences. In this sense, psychology is exactly like any other science. The report with which psychologists are provided may be about rats running a maze, physiological processes, test results, or even the scientist's own introspection. But the element of all of this that is useful is the report that is open to public inspection.

The particular task which the psychologist has taken for himself is that of trying to make some sense of behavior. But we have just seen that, as scientists, psychologists must concern themselves with events that are publicly observable and, therefore, verifiable. This restriction on the activities of psychologists raises a question: Just what kinds of observations can they make on behavior that are of this sort? The answer to this question comes in two parts. First, psychologists have found (just as other scientists in other fields have found) that human behavior is so complicated that it is impossible even to talk about all of the activity of an individual at any one time. So, in practice, they restrict their observation to limited portions of behavior called responses. Second, even responses cannot, without the aid of photographic assistance, be recorded in detail. So, their reports of behavior typically deal with some abstracted characteristic

* This (somewhat revised) is a paper read before the Rhode Island Philosophical Society at Brown University in 1949. Its purpose is to present in simple language the ideas that are basic to modern behaviorism. A more technical development of these ideas (and probably some of the happier phraseology) can be found in the writings of Bergmann and Spence. The sophisticated reader will note that this account underemphasizes the deductive aspects of theorizing. The omission does not mean that I regard this process as unimportant. It means, instead, that the purposes of this paper could be achieved in a more straightforward manner by stressing less technical matters. I am indebted to Professor Bergmann for reading an earlier version of the manuscript, and to Professor C. J. Ducasse of Brown University, whose interest in and criticisms of this paper have led to a number of basic improvements.

like the speed or the magnitude of the response.

Having stated the general manner in which the psychologist views behavior, a word must now be said about a primary assumption, which is that these various aspects of behavior are related to events in the person's past and present environment. Although the collection of variables in this category might better be called antecedent and attendant circumstances, the psychological convention is to call them stimuli. When the psychologist speaks of stimuli he usually is referring more or less broadly to events or changes in events in the environment. So far as these stimulus variables are concerned, they are so numerous that no attempt to enumerate them here is possible. A few examples will have to do. In the field of learning, for one example, the efficiency of a learned reaction is known to depend in a regular fashion upon the number and spacing of practice trials, upon the amount of reward the subject is given for making the response, and upon many other variables. In vision, what a person sees (really what he *says* he sees) is known to depend upon the area of the visual stimulus, upon its brightness, upon the duration of its presentation in the visual field, and so on. One thing that should be noticed about these particular examples of environmental events is that they are quantifiable in terms of some standard physical measuring scale. Not all variables with which psychologists have to deal are yet subject to measurement in any very respectable sense of the word. For the kind of information that is needed to build a science of psychology, however, quantification of these independent variables is a necessity of which psychology is acutely aware.

The final task in the development of a behavior science is simply that of stating the relationships that exist among the variables that have been isolated. What psychologists are after is a set of laws of the general type: $R = f(S)$, where R is some aspect of a response, S is a stimulus event and f represents the functional relationship. These are what I will call S-R laws. It should, of course, be realized that the formula, $R = f(S)$, is highly schematized. Behavior is seldom predictable from knowledge of a single S-variable. The S in this equation stands for an indefinite number of antecedent conditions which in actual practice will have to be discovered along with their relevance to behavior.

Because of this complexity in the determination of behavior, the S-R laws have turned out to be very difficult to discover. Where this has happened, psychologists have sometimes had more success using a different approach, in an attempt to discover the relationships that exist among *response* variables. As a convenient example of this, one may take the very important work of the clinical psychologists and the psychological testers. They almost never deal with the S-R type of relationship at all. Instead they use what are sometimes called R-R laws. The distinction between these two types of relationship is shown in the following example. There is some rather good evidence to suggest that an individual who is frustrated in his various strivings develops a tendency toward aggressive behavior. For purposes of exposition, it can be assumed that this is a well-established relationship. On this basis, if one knew that an individual had been frustrated many times in his efforts to achieve important goals, it could be predicted that he would show aggressive behavior. In this case, an S-R law would be used to make the prediction. The determining S-variable is the frequent blocking of goal-directed behavior, and the dependent response variable is the aggressive behavior. If it were known that aggressive behavior in some form always occurs as a consequence of frustration and that it never occurs in the absence of frustration, there would be some point in saying that aggression occurs *because* of frustration. Furthermore, given a control over the person's environment and a knowledge of the S-R law, the amount of his aggressive behavior could be controlled.

A clinical psychologist, on the other hand, might give this same person a psychological test and come out with the statement that he has strong aggressive tendencies and may be expected to show a great deal of hostile behavior on many occasions. This prediction (which would be as correct as the other one) would be made in terms of a different kind of law. What the clinician has done is to observe a small segment of the individual's behavior in a controlled situation called a test, and, from his behavior in this situation, to make a prediction about other behavior. His prediction is made in terms of an $R_2 = f(R_1)$ kind of relationship, in which R_2 is the predicted aggressive behavior, R_1 is the predicted-from-test behavior, and the function is a correlation coefficient. These R-R laws are diagnostic laws, and carry no implications of causality. (The statement that the person tested is aggressive because the clinician has given him a test is obviously absurd, at least until such a time as the clinician's services are more expensive and, therefore, more frustrating than they are now.) Furthermore, the R-R laws provide no control over behavior in the absence of further information of the S-R type.

The commodities in which the psychologist deals are, then, two types of relationship which I have called S-R and R-R laws. The precise statement of

a large number of such laws would be extremely valuable, but it would not make psychology a high-order science. What would still be needed would be the collection of the *S-R* and *R-R* relationships into some more orderly or integrated kind of formulation. In actual practice, it has been found useful to attempt the formulation without the comprehensive set of empirical laws it is hoped eventually to integrate. Such attempts at formalization are called systematic psychology or psychological theory.

The form that psychological theorizing has taken has been toward the development of intervening state variables which are considered as standing between S and R or between R_1 and R_2, depending upon the kind of relationship. The intervening states that have been postulated have been of two general sorts, physiological states and hypothetical constructs. If we consider first the type of intervening variable used by the more physiologically minded psychologists, the paradigm in terms of which they attempt to explain behavior may be expressed in the following way: *S-O-R*. Stimulation according to this scheme is thought of as producing some physiological or organic change in the individual. And this physiological change, in turn, is thought of as being responsible for whatever behavior is being observed. Such psychologists investigate the relationships between neural, glandular, circulatory, and muscular events, on the one hand, and behavioral events on the other.

Another large group of psychologists make use of the hypothetical construct type of intervening variable in their explanation of behavior. The paradigm, in terms of which such psychologists work, is the following: *S-H-R*. Antecedent stimulus conditions are thought of as defining a hypothetical intervening state in terms of which responses are predicted. Examples of such hypothetical constructs in fairly general use are: habit, motivation, fatigue, attitude, symbolic processes, and the like. About these hypothetical constructs, there are a number of things that need to be said. The first of these is that there is no reason in the world to assume that they *must* have any actual existence inside the organism or anywhere else. Some of them, at least, are analogous to the physicists' concept of velocity which derives its meaning from its definition: $V = S/T$. The physicists do not spend much time worrying about what or where velocity really is. Psychologists, on the other hand, spend a good bit of time looking for the neurophysiological events which they think *have* to exist and to correspond to their concepts, however imperfect. What is worse, they spend even more time speculating about them. There are just two comments that I wish to make about this. One is that, although all behavior probably has its neurophysiological aspects, mere speculation about them is a fruitless procedure. The other is that, although physiological *information* may someday turn out to be useful for psychology, there is nothing in the program of psychology which makes such information indispensable. For all practical purposes, it is possible to construct a science of psychology in which the organism is considered as empty. For my own part, I can conceive of a psychology based on stimulus and response events entirely, one in which the existence of the organism is a completely unimportant fact. The scientific account will, after all, deal with behavior in the abstract. Such a science no more needs to refer to the organism than the science of gravitation needs to refer to stones or to the Leaning Tower of Pisa.

Whether the constructs that are introduced between stimuli and responses are physiological or purely hypothetical, they have to meet certain criteria to be acceptable. The most important of these criteria can be summed up in one word: *meaningfulness*. Intervening variables must be meaningful in two ways. The first is what has been called *operational meaning*. The meaningfulness of a concept in this sense depends upon the adequacy of its definition. A concept may be said to be adequately defined providing there exists a set of defining statements for it which lead eventually to phenomena in the sensory experience of the scientist, and providing further that these statements are such that any properly qualified and equipped person can put them to a test. This requirement is no more than a particular application of the more general criterion of public observability.

Concepts for which no such definition is *possible* are of no use to science. This is not, however, to say that concepts which have never been adequately defined never will be. They may or may not be. Concepts such as mind, consciousness, and experience are in the class of intervening variables for which no satisfactory operational definition exists. This is in part because of the complexity of the phenomena involved, and in part because of their inherent privacy. But this state of affairs does not demand the conclusion that respectable definition is impossible.

On the other hand, an operational definition of these terms (assuming it is achieved) does not guarantee their utility. The second kind of meaningfulness remains to be demonstrated. This point can be made clear with the aid of an illustration. I can define a construct as follows: Kappa (for Kimble) = the square root of the number of hairs on my head divided by my diastolic blood pressure minus the length of my great toe. This concept

Kappa, although its operational definition is impeccable, has not been used much in psychology because, so far as we know, it has no meaningful relationship to any of the events with which psychologists are supposed to be dealing. In this second sense, a concept is meaningful if it is related to behavior, or aids in its prediction.

Just as there are two major kinds of laws with which the psychologist deals, there are two kinds of operations in terms of which a concept may acquire operational meaning. The first of these is by definition in terms of antecedent stimulus conditions. The second is by an inference from behavior. To illustrate, let us consider a case in which both types of definition have been used and return to the example of aggression. It has already been pointed out that predictions of aggressive behavior might be made either from a knowledge of an individual's history of frustration or from his behavior on a test. Using exactly the same evidence, there can be developed two different definitions of the concept aggression. In terms of antecedent conditions, aggression can be defined as a positive function of the number of frustrations a person has experienced. In terms of behavior, it can be said that a person is aggressive if his pattern of test responses is so and so. The one thing we cannot say is that we now have two different definitions of the "same thing." Different definitions define different concepts. In psychology, especially, there is the need to be cautioned against the tendency to treat things as identical just because they have the same name. Whether the frustration-produced aggression has anything to do with the test-defined aggression is an empirical question which can be decided by research.

As I see it, just one final point needs to be made with respect to the science of psychology as it is developing. It must be presented as a sort of confession. Not all psychologists agree with the analytical, behavioristic (what James would have called "tough-minded") sort of analysis I have been presenting of the field. In psychology, we hear a great deal about "the total situation," "the whole child," "the whole personality," and "global intelligence." Psychologists who use these expressions object to the type of account I have given, chiefly because of the analytical approach which it implies. This cold analysis, they say, is wrong because it fails to capture the warmth and vitality of human behavior as we know it in our common-sense, firsthand experience. Unfortunately for common sense, that is how it is with science. Common sense tells us that the world is flat; science says that it is round. Our firsthand experience tells us that the sun moves slowly around us; science says that we are moving around the sun, and at a breakneck speed. Direct observation tells us that the chairs in which we are sitting are solid; science says that they are mostly empty space. The mere fact that some line of scientific argument produces a description of the world that fails to correspond to our naive experience of it is no obstacle to that argument. And if there are aspects of behavior that cannot be handled in the way I have been describing, I know of no way of finding this out without giving it a try.

At a somewhat different level, the opponents of behavioristic psychology sometimes object that the analysis of behavior destroys its essentially continuous or integrated character, and robs it of its wholeness. They now propose what they call a field theoretical approach to human behavior and very bitterly deplore what they call the *mechanism* of the sort of argument I have been expounding. They say that their new field theories stand to the more orthodox psychology as does the theory of relativity to Newtonian mechanics. The adequacy of this analogy, of course, depends upon some understanding of what the term, "field," means in physics. It seems to have been used in two rather different ways. Basically, by a "field," the physicist means a system of interrelated variables that differs from other systems of physics partly in the kind of calculus which expresses the interdependencies. Unfortunately, the mathematics are difficult, even for a physicist. So, for their nonmathematical brethren, the theoretical physicists have constructed models (as, for example, the Rutherford-Bohr atom) in an attempt to convey what is involved in their equations. Since the so-called field theories in psychology consist of anthropomorphic models in this second sense, it seems to me that they cannot be called field theories in the sense that the theoretical physicist uses the expression. On the other hand, if one wished to consider as field theories approaches to psychology that come closer to the actual mathematical statement of the interdependencies involved, then the very people who bear the stigma, "mechanistic," would have to qualify as the greatest field theorists of them all. Finally, it must be pointed out that science is concerned with the regularities in the world; that is, with events that repeat themselves. Whole situations do not repeat themselves, at least not very often. Analysis of the elements common to somewhat different situations will be absolutely essential in order to achieve a science of psychology.

This, then, is what the science of psychology, as it begins to be rather definitely defined, is like. It is concerned with the prediction of behavior either in terms of other behavior or else in terms of antecedent stimulus conditions. Its proponents are

trying to develop a consistent body of scientific knowledge expressed in terms of whatever lawful relationships may be found to exist among these variables. Because of the tremendous complexity of this task, it has been necessary to use concepts that are considered formally as intervening between stimuli and responses in one case and between predicted responses and predicted-from responses in another. Furthermore, different psychologists develop different kinds of concepts. Some use mechanistic ones, some use field theoretical ones, others think physiologically. Which of these approaches is the best cannot be decided on the basis of *type* of concept. What really counts is the extent to which these concepts aid in the primary task of psychology —the understanding of behavior.

LYCAENOPS

The mammals, as indicated by Dr. Rodbard in the present issue of this journal, may owe their evolutionary success, at least in part, to their warm-bloodedness. This character, with its accompanying mechanisms that permit maintenance of a constant internal environment, has undoubtedly played an important role in the successful establishment of mammals in a variety of external environments. Paleontological evidence strongly indicates that the mammals evolved from the mammal-like reptiles. These, which comprise the reptilian subclass Synapsida, actually were among the first reptilian groups to appear on the earth; indeed, they had already passed their zenith before the coming of the first dinosaur. Primitive mammal-like reptiles made their entry in late Pennsylvanian times and underwent their great evolutionary radiation during the Permian period. By the Mesozoic era they were on a marked decline, and disappeared in the Jurassic period. The mammals are believed to have arisen during the Triassic period from some group of generalized mammal-like reptiles. Although the precise ancestors of the Mammalia are as yet unknown, members of the suborder Therapsida of the mammal-like reptiles, which flourished from middle Permian to middle Triassic times, practically bridged the structural gap between reptiles and mammals: thus, e.g., the lateral surface of the skull, the palate, and the lower jaw are largely of the mammalian pattern; the teeth, instead of being relatively simple cutting cones, as in typical reptiles, exhibit differentiation into incisors, canines, and cheek teeth, as in mammals; and many features of the limb skeleton are distinctly mammalian and strongly suggest a mammalian type of posture and locomotion.

The picture on the cover of this issue is of a restoration of the mammal-like reptile, *Lycaenops,* in a late Permian landscape. This restoration, made by J. C. Germann, under the direction of Dr. Edwin H. Colbert of the American Museum of Natural History, New York City, appears through the courtesy of the American Museum of Natural History. *Lycaenops* is a member of the order Therapsida, suborder Theriodontia, family Gorgonopsidae. It represents one of the more primitive theriodont types probably on, or near to, the line of evolution that led to the mammals. For further information, interested readers can consult such publications as *Vertebrate Paleontology,* by A. S. Romer, University of Chicago Press, Chicago (1945); *The Life of Vertebrates,* by J. Z. Young, Oxford University Press, New York (1950); and The Mammal-like Reptile *Lycaenops,* by E. H. Colbert, in *Bull. Am. Museum Nat. Hist.,* **89,** 353 (1948).

SCIENCE ON THE MARCH

OPERATIONS RESEARCH AND INDUSTRIAL ENGINEERING: CONTRAST AND RESEMBLANCE

MORE than a half-century ago Frederick W. Taylor conceived and vigorously espoused his "Principles of Scientific Management." In his justly famous book by that name he described the dramatic experiment with Schmidt, the pig-iron handler, whose daily output was increased from 12½ to 47½ tons by application of new techniques of work measurement and wage incentive.

Taylor's "scientific managment" aroused great controversy during his lifetime and, indeed, still does, as many disputes between labor and management will attest. Despite this extensive controversy, the techniques of time study and the application of wage incentives have spread over a large portion of American industry. Most of what today is called industrial engineering rests upon a foundation of work measurement and wage incentives and, in common usage, there is an inevitable association: *time study* to a considerable extent defines industrial engineering and *industrial engineering* always connotes time study.

In devotion to the techniques discovered and used by Taylor there has been an absence of consideration given to the organizational role that he filled. In his celebrated studies of pig-iron handling, shovelling, and metal cutting, Taylor and his associates were in a unique staff relationship: they were, perhaps for the first time in formal organization, assigned to the task of analyzing operations with the objective of effecting improvements in them. This was and is a role which has no exact counterpart among other staff units. Personnel departments, accounting, finance, maintenance, even research and development, presumably are expert in their respective fields but their tasks primarily are *doing* things in these areas, not in analyzing and seeking to change the operational activities of others. Taylor's unique organizational role was a creation just as significant, perhaps much more significant, than the development of techniques in time and motion study.

Since Taylor's day, industrial engineers unfortunately have followed somewhat slavishly in his footsteps. The Gilbreths contributed notably to motion study and Gantt's famous chart and his insight into human problems were futher advancements of great importance to industry. But, by and large, industrial engineering has been devoted too narrowly to setting time standards for wage incentive application.

II

During World War II a new approach to the analysis of action problems achieved dramatic success. Since the War that approach has come to be called *operational research* in England, the country of its birth, and *operations research* in the United States. In both countries postwar development has been rapid, with attendant controversy that is reminiscent of the early days of scientific management.

Some of this controversy takes place among the operations researchers themselves, who are thus far unable to agree upon a uniform definition of their activities. They do agree that their mission is the study of operations with the objective of advising administrators in decision making, and they do agree that their approach to the study of action problems involves the use of multidiscipline teams, suitably assembled for each particular problem. Thus their staff role is the same as that of industrial engineers but their approach is very much broader.

On the content and scope of the research teams there is much disagreement. One "school" of operations research believes that the disciplines represented should be limited to those of the physical sciences and that counsel given to administrators should be restricted to those findings which can be quantified with precision. Their usual objective is reduction of the problem to a mathematical model. Other practitioners also favor mathematical models but believe that the multidiscipline approach should embrace *all* fields of scholarship, the biological sciences, the social sciences, and the humanities, whenever action problems involve elements of such fields. This group believes that precise, less exact, and even intuitive information may be supplied to the line administration, provided the nature and tolerances of each class of information are identified. Since almost all administrative decisions involve elements which defy quantification, and since every action problem in

human affairs involves the emotions and aspirations of people, the broader scope of the second point of view seems much more realistic to this observer.

Other controversy, centered chiefly in the academic world, concerns allegations that operations research is a new and separate discipline, that the whole is different from the sum of the parts represented by the multidiscipline approach. In these arguments one may detect some measure of defensiveness on the part of operations researchers who desire academic ties and academic respectability. Perchance there is also a measure of intellectual snobbishness on the part of academicians who disdain contact with mundane action affairs.

III

Beyond these arguments, and much more important than the disputation, are significant facts. Operations research occupies the identical, unique organization relationship as industrial engineering, but its analytical staff role is capable of being infinitely more fruitful because its investigations are not bounded by work measurement and incentive concepts. In fairness to industrial engineers it must be said that they too have broadened their vision but, still, operations research literally compels consideration of many facets of action problems which traditionally have not been studied by industrial engineers.

The importance of this unique and now augmented organization relationship is enhanced by the undisputed fact that operations research has brought to bear upon action problems a new group of people, scholars and scientists who are imaginative and resourceful, who have fresh, tradition-free points of view, who are capable of putting to new uses tools of science and the humanities. There are at least two consequences of this entry of new people into a new field: the scholars themselves are discovering the fascination and the difficulty of problems in the world of affairs, and they are achieving dramatically successful results in their operational studies

Here too there are analogies to the early days of industrial engineering. Operations research teams on more than one occasion have proclaimed that improvements in operational situations have derived from application of the "scientific method." What actually took place was the organizational assignment to operations analysis of persons trained in science; what they thereupon did was inventive, ingenious, and altogether commendable, but it was not the "scientific method" in so far as that term implies controlled experiment, measurement and quantification, and reproducibility. Frederick Taylor yielded to just the same impulse, in his day with better cause, when he called his approach "scientific management" because it was analytical and systematic.

Comparison also can be made with the "Jack Horner" attitude too often shown in the past by industrial engineers when they have boasted of increases in productivity and reductions in costs by ignoring or suppressing negative attributes of their work. Not without cause is "efficiency expert" a term of opprobrium. The entry of quacks into the field, or the substitution of vanity or boasting for intellectual honesty, can have the same detrimental effect upon the future of operations research. The practitioner who promises to reduce the ramifications of an organization to a magic mathematical model is of the same harmful breed as the efficiency expert who promised to double production by application of a time study-wage incentive formula.

IV

These admonitions and seeming criticisms are by no means trivial but they are relatively minor. The fact remains that operations research brings new people of great ability and new concepts of great value to the study of problems in the world of affairs. It is often said that science and technology have outstripped our capacity for the peaceful assimilation of new discoveries. It is also said that the humanities and social sciences must meet this need by the rapid extension of their own frontiers of knowledge. Operations research will provide no panacea for the social problems created by science, but the union of scholars and action problems is certain to be beneficial.

Economically, organizations of all kinds need staffs whose task is the analysis and improvement of operations, and realization of this need is just as important as the techniques used or the people employed, whatever they may be called. Despite its history of controversy and the narrowness of its approach, organizations have profited from industrial engineering. It is fair to hope that even greater gains will come from the imaginative and resourceful teams of operations research.

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BOOK REVIEWS

BIRDS AND BEASTS

Birds and Mammals of the Sierra Nevada. Lowell Sumner and Joseph S. Dixon. xvii + 467 pp. Illus. $7.50. University of California Press, Berkeley and Los Angeles. 1953.

THIS book is far more comprehensive than the title indicates. The short introduction and the first nineteen pages should be required reading for all, preferably early in life.

The introduction points out changes that man is bringing about on the face of the earth and it raises questions on which all should ponder, for they are changes that affect not only the plant and animal life but they have a great and often adverse effect on man as well.

The first chapter, Wildlife Policies and Problems, sets forth the factors that are altering plant and animal life in general wherever so-called civilized man conducts his developments or exploitation, and quotes carefully formulated policies that have been developed by the U. S. National Park Service for administration of National Parks and other areas under its jurisdiction.

From this, we soon see that the principal section will treat of the birds and mammals of the Sequoia and Kings Canyon National Parks, including the General Grant Grove section of Kings Canyon National Park, rather than the entire Sierra range. However, the area has been intensively studied by careful workers and it is typical of the entire Sierras, hence the title is not misleading. A large black-and-white map shows boundaries of the area, some topographic features, trails, and other man-made features.

The second chapter, Life Zones, not only gives the customary specific information regarding the characteristics of the various life zones in the Sequoia-Kings Canyon region with which biologists are familiar, but it gives simple explanations regarding life zones that enable the novice to understand life zones in general and their role in studies of biological subjects. Another large black-and-white-hatched map shows the life zones of Sequoia and Kings Canyon National Parks.

The birds are treated in pages 20 to 275. Each species or subspecies is discussed in three sections: a brief Description, Habits, and Park Status and Record. The descriptions are sufficient to enable the lay student of birds to be reasonably certain of his identification in the field, and it omits the long lists of measurements and technical descriptions that are useless unless one has specimens at hand. Discussion of habits of the various birds properly differ considerably in length. For birds that are well known the accounts of habits are brief, but for those forms whose habits are not so well known the author has used sufficient space to set forth new facts or peculiarities that are of special interest. In Park Status and Records, if the form is of regular and fairly common occurrence it is treated briefly. If it is rare or there are noteworthy records regarding the form, the account is more comprehensive. One hundred sixty-four forms of birds are discussed at length. Twenty-six forms are treated briefly under the chapter Vanishing Species and Questionable Records, Birds.

Seventy-eight forms of mammals are treated in a similar manner on pages 276 to 467. This includes seven forms under the chapter Vanishing Species and Questionable Records, Mammals. The discussions of the mammals are longer, on an average, than those of the birds and set forth many interesting facts that are not generally available or known. The average sizes of mammals are given in terms of total length and length of tail. It is pleasing to note that body weights are also given, a feature which is rare in books on animals. There is a two-page tabulation that should assist both the layman and the zoologist to distinguish between the forms of chipmunks (Eutamias) that frequent the area. The mammal section shows the meticulous thoroughness of Joseph Dixon, its author, and includes facts based on his long, painstaking studies in the field that might easily be missed by zoologists interested only in taking specimens.

The forty-six halftone pictures of animals, as well as views of their habitats, are uniformly excellent, an agreeable step forward. Eight color plates, three of birds and five of mammals, by Allan Brooks, are beautiful additions that will assist both the novice and the professional zoologist. On the three bird plates are excellent representations of seventeen different kinds of small birds that should go far in assisting all observers to distinguish between some forms that sometimes appear to be very similar.

The section headed references lists not only published works that are standard references of zoologists but also memoranda and field notes, and manuscript reports of the National Park Service employees, which are not generally known to exist. The citation of these not only serves to verify the statements in the work but will focus the attention of future workers in this and other areas on the fund of information that exists in certain administrative files.

The index is limited to vernacular names, which are probably so sufficiently well known that the omission of the scientific names will not be a handicap in using the book.

When I was invited to review the book I was pleased because I had been well acquainted with Dixon and knew that his long, careful sympathetic study of the mammals of California would bring much of value to any of his writings. Furthermore, while I have not had the pleasure of being well acquainted with Lowell

Sumner, the author of the bird section, I have heard of him so favorably that I was certain his portion would be a valuable contribution.

In addition to the basic parts played by the two authors of this work, the book has had the benefit of the careful scrutiny and advice of wildlife administrators and zoologists of the National Park Service and the Fish and Wildlife Service, as well as the many capable California zoologists with whom the authors were associated for many years.

By the time I had finished with the examination of the book for this review, I was delighted. The work is not only well done but it is made understandable and interesting to the layman, and it carries educational and conservation values far beyond those found in most treatments of local animals.

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The Birds of Mexico. Emmet R. Blake. xxix + 644 pp. Illus. $6.00. University of Chicago Press, Chicago, Ill. 1953.

THE increasing travel to Mexico by tourists interested in birds has emphasized the need for a compact, accurate, and complete book on the rich avifauna of that country, so written that the informed amateur bird student could use it readily to enable him to identify the native birds. Blake's book is the answer to this need, and it gives every promise of being a good and usable answer. In the space of a volume small enough to be carried conveniently in one's baggage, the author has treated of the nearly one thousand species and more than twice that number of subspecies of birds recorded from Mexico. It includes not only the resident birds, but also those that migrate there from the United States and Canada. Approximately a third of the species are figured in black-and-white pen sketches, while convenient keys enable the reader to distinguish related forms. Each species is briefly described in the text, and the range of each, and even of each race of each species, is tersely stated.

This is a manual for identification, and it wisely does not attempt to go into such matters as habits, nests, or eggs. To do this would have so greatly increased its bulk as to make it a library rather than a field book. Because of the extent of the fauna it covers, the book should prove useful to travelers throughout Central America, although there are many forms of birds in the other Central American republics not included in the Mexican list. They are, however, related closely to those treated in this volume.

The book is well printed on good paper, with clear type, and is well indexed and bound. It may be recommended to all bird-minded travelers.

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SEA NOTES

Waves and Tides. R. C. H. Russell and D. H. Macmillan. 348 pp. Illus. + plates. $6.00. Philosophical Library, New York. 1953.

LAY readers have found it difficult to find popular works on various aspects of oceanography which contain much more than a poetic appreciation and sensational treatment of "the mysteries of the deep." For those who want a sober, accurate, elementary presentation of the known facts and ideas about waves and tides, the work by Russell and Macmillan will come as a very welcome surprise.

In recent years waves have received more attention from research workers than tides, and for this reason the first part of the book by Russell is interesting reading and covers material not included in Cornish's books. A number of popular works on tides have been written in the past, for example, those by G. H. Darwin and Marmer, and more recently the Admiralty Manual of Tides by Dodson. The second part of the book on tides by Macmillan covers much the same material as these works but in less detail and in a slightly more popular fashion.

The book can be safely recommended as a gift to mariners and yachtsmen, the audience for which it is evidently intended. It is not likely to be of much interest, of course, to the professional physicist or geophysicist.

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Fishery Science: Its Methods and Applications. George A. Rounsefell and W. Harry Everhart. xii + 444 pp. Illus. $7.50. Wiley, New York. 1953.

IN spite of the great flood of papers and bulletins on fisheries, their problems and procedures, this is the first attempt to prepare an introductory textbook of the subject. It is, according to the authors, a treatise on methods, and "the real purpose is to present the problems that confront the administrator, the research worker, and the student, and to show how to go about solving them." It is assumed apparently, that the user of this text understands basic ecological principles and elementary statistics. Although the book is in many ways an elementary text, it is in other ways an advanced textbook because of this assumption that the reader possess enough background information to follow the rather brief presentations of some of the complex concepts of fisheries and to recognize omissions where they occur.

The authors do not get around to "problems" until the final chapter; the bulk of the book is given over to methods of investigation and analysis of data, appliances, fishways, gear, and stocking policies. It might have been better to state the problems first and then discuss the techniques used in studying them. Because of this obvious concern with means, this book cannot stand by itself as a textbook of fisheries although the

publishers claim it to be a "complete text for all phases of fishery science." Some topics are sketchily treated, and a few are omitted altogether. The most conspicuous omission is that of any discussion of salmon hatcheries although there is a chapter on "salmonids." The authors may well have felt it best to avoid this thermonuclear potato, but the compilers of a text must occasionally venture beyond the angels if they would strive for comprehensive coverage.

As a first approximation, however, this book provides a useful summary of the methods and devices at the disposal of the fishery worker, whether fresh water or marine, and fills a longfelt need for a concise presentation of widely scattered materials. Much more is included than omitted, and its use as a textbook in fisheries methods should soon provide the authors with new material for the improvement of subsequent editions.

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Between the Tides. Philip Street. 175 pp. Illus. + plates. $4.75. Philosophical Library, New York. (Printed and bound in Great Britain.) 1953.

IT is fair that a book be judged in terms of its objectives. Those of *Between the Tides* are clearly stated by its author: "This book is intended primarily for the young naturalist. It introduces him to the rich variety of life on the seashore in the hope that it may stimulate him to further study with the aid of more complete guides written for the adult naturalist. The reader I have had in mind . . . is of no particular age [but] usually he will be in his 'teens' . . . " This is a good objective, fairly stated, and on the whole well carried out. *Between the Tides* deals with the common and obvious animals (and seaweeds) of the seashores of Great Britain. Common names are used throughout, plus scientific names in picture captions. In addition a glossary of vernacular and scientific names is given in an appendix. There is an adequate index. The language is nontechnical, and this reviewer found the style interesting, as did his ten-year-old son. The book is illustrated by a large number of good black-and-white photographs, supplemented by line drawings. The text describes the natural history of the forms covered. The illustrations are said to be adequate for most identifications (the reviewer, being unacquainted with the British marine fauna, cannot judge the latter point).

This book is suitable for boys and girls in their early teens, and many parents visiting the seashore would find the book interesting and useful. It is, of course, much less likely to be useful in this country than in Britain, since it deals with a local fauna. The work is not intended for the adult biologist, nor even for the informed amateur naturalist—many other well-known books on the sea shore and its fauna are available both in this country and in Britain. Mr. Street's book is intended to fill the evident gap between such adult books and the large number of "children's books" on the subject. Without claiming great originality, he has done a good job. Books of this type are needed for the coasts of the United States.

By way of criticism: although the book is to be commended on its photographs, it should be noted that the line drawings are poor. There are a few errors of fact and a good many over simplifications, the latter in the main justifiable in a work of this sort. The price of $4.75 seems decidedly high for a book of this size—presumably in Britain where most of the sale must be expected it would cost less. But, all in all, a good book.

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ABORIGINAL AMERICA

Archeology of the Eastern United States. Edited by James B. Griffin. x + 392 pp. 205 Figs. $10.00. University of Chicago Press, Chicago. 1952.

THIS comprehensive testimonial volume was prepared in honor of Fay-Cooper Cole, Chairman Emeritus of the Department of Anthropology at the University of Chicago, by twenty-six of his former students. There is an introductory essay on "Twenty-five Years of Archeology in the Eastern United States" by Carl E. Guthe, long a professional colleague of Dr. Cole's.

The work contains, in addition to Dr. Guthe's contribution, twenty-eight articles, all save six devoted to the summary treatment of archeological areas east of the Mississippi River. These exceptions comprise a classification of Indian racial varieties of North America by Georg K. Neumann, a consideration of ethnological correlations with archeological remains by Fred R. Eggan, a survey of historic site excavations in the United States by Jean C. Harrington, an evaluation of dendrochronological studies in the Mississippi Valley prepared by Robert E. Bell, a concise synthesis of eastern United States culture periods, and an appendix summarizing the radiocarbon dates for the same area, the last two by the editor.

The numerous illustrations, based on photographs and drawings, were prepared by several persons, some of whom are mentioned in the preface, and they range in quality from good to fair. For certain areas, due no doubt, at least in part, to spatial limitations, the illustrations only partially cover the major trait characteristics of the cultures described.

As stated in the preface, "Unfortunately, the chapter which would have discussed the geographical background of this broad area and its influence on the prehistoric cultural development was not submitted, nor was the section on the Central Plains completed."

About two-thirds of the articles were received by the editor in 1947, all but three of the remainder in 1948. Only two were revised to 1951. Accordingly they are not equally up-to-date. Moreover, they are of unequal value, owing to the additional fact that for some areas the data were drawn largely from older, and often

inadequate, published sources. For the most part they do incorporate the results of the newer field work published or otherwise available at the time of preparation of the individual reports. Editorial footnotes frequently comment on changing opinions and discoveries up to 1951.

From the viewpoint of the professional student, who will, in general, find this book of value as a summary, what appear to be numerous instances of overediting will seem regrettable. For the nonprofessional it may offer, for the most part, difficult reading, and the illustrations may prove insufficient for his purposes.

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TOURING THE MOON

The Moon. George Gamow. 118 pp. Illus. + plates. $2.50. Schuman, New York. 1953.

A Guide to the Moon. Patrick Moore. 255 pp. Illus. + plates. $3.95. Norton, New York. 1953.

YOU should keep from the young the two books before me for review, and from their elders also, unless you are willing to take the rather expensive consequences. Telescopes suitable for exploring the surface details of the moon can cost from two or three hundred up to many thousands of dollars. The impulse to stride over the plains and climb the steep hills and steeper jagged mountains of the moon will be very strong after an acquaintance with these new rocket-age volumes, which are preparing earth-bound man for the shaking off of his terrestrial shackles.

Dr. Gamow's little book is aimed at the beginner's level and is filled with those clear explanations of technicalities and the graphic drawings and word pictures for which the author is well known through his recent popular books on the origins, ages, and destinies of the earth, sun, stars, and of the total universe. As a distinguished theoretical physicist, he writes on the moon, you might say, from the outside—outside the moon and the lunar field. Patrick Moore, on the other hand, writes from the inside; he writes for the mature and ambitious reader, and still does it so simply and smoothly that the reader can start with no scientific knowledge whatever and come out as something of a lunar expert. With untechnical thoroughness he discusses the problems of the lunar atmosphere, of rockets to the moon, of the origin of the craters, and of the establishment of a lunar colony. He has produced an extraordinarily good book which cannot fail to arouse enthusiasm for knowing more about this tantalizing nearby planetary pal of the earth. Moore's specialties include general astronomy, interplanetary travel, lunar research, and the hope for freedom from idiotic wars that politicians thrive on but which deter man's intellectual adventures. He takes it for granted that before long man will fly around the moon (Gamow agrees), and later actually wander over the moon's spectacular surface. His book is rightly a guide to the hundreds of named special features on the moon, and equally a guide to the best knowledge of the past and present about our satellite.

The earth's isolation from its companions in the solar system began to end, as Moore points out, on March 16, 1926. That was the date when the American physicist and rocketeer, Robert Goddard, pioneering in the field, fired his first effective shot. From then on it was clear that space flight, beyond the atmosphere, was practical. With maps, photographs, and detailed indices, Moore is preparing the lunar traveler for the future landings. Another goal of his volume, which he attains in part, is to spell out for us what obstacles we must overcome in the early days of our interplanetary operations.

Three lunar characteristics, heretofore not generally accepted in the astronomical textbooks, are presented by Moore with convincing argument based on competent observation. The first is that the moon is not wholly dead and changeless. Within the past century or two several visible topographic changes have occurred, the result perhaps of landslides, meteor impacts, seismic shock, or vestigial volcanic activity. The second is that a thin atmosphere exists on the moon, at least fleetingly from time to time. This atmosphere occasionally dims or hides the surface features in certain craters and clefts. The atmosphere is of course not hydrogen or oxygen or other light elements, since the moon's gravitational power could not hold such material, especially when heated by the 214° Fahrenheit sunlight of midday; but the atmosphere may be sufficient to protect future hypothetical human colonies from infalling meteors.

The present reviewer has proposed that a continuously regenerated atmosphere of argon gas must exist on the moon as a result of the radioactivity of isotope 40 of the undoubtedly prevalent element potassium in the lunar rocks. The argon combines with nothing, is fairly heavy, and would not easily escape; but it will not be useful to the lunar expeditionary forces except perhaps in helping provide protection from small meteors.

The third point made by Moore is really only the lending of weight, based on observation and careful physiographic analysis, to the hypothesis that almost all the craters of the moon are attributable to volcanic action and not to meteoric infall.

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THINKING MAN

The Common Sense of Science. J. Bronowski. 154 pp. $2.00. Harvard University Press, Cambridge. 1953.

THIS book is the story of the evolution of science as a part of the cultural history of western civilization. The author believes that the modern concept of science as something separate and distinct from the arts is false and that it is essential to correct this misunderstanding. To achieve a common understanding,

the ideas and methods of science must be expressed in a language understandable to both scientist and non-scientist.

The author traces the history of science from Aristotle to modern times as the history of three dominant ideas. These are the idea of order, the idea of causes, and the idea of chance. The author begins with the idea of order and shows how the Scientific Revolution of the 17th century changed the concept of the universe from a purposeful, willful universe to a strictly deterministic machine in which every effect follows automatically from a specific cause. The discovery of quanta and the principle of uncertainty in turn changed this idea to one of a statistical universe in which chance plays a significant role.

The methods of science are pictured as being an outgrowth not only of a common behavior pattern of man, but also of a common pattern of adaptation of all living things to future probabilities. The mutations of organisms are unconsciously produced solutions to the organisms' problems of adaptation. They constitute a prediction of the future environmental conditions that these organisms will face. The empirical test follows and the organism lives or dies depending on how well it predicted the future. Common sense behavior of man follows the same pattern. His ideas are predictions that guide his behavior. If he predicts the future correctly he survives and prospers, if not he suffers and may die. The theories, laws, and hypotheses of science are similar though more refined and systematized predictions of the future. The concept of uncertainty enters because no prediction can ever be made with absolute certainty. Statistical probability is as close as one can ever come to certainty.

The significance of value to science is illustrated by the relation of the value of truth to scientific endeavor. Throughout the book the close relationship between the growth of science and the cultural environment of the time is stressed. This is an important book and deserves to be widely read.

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Readings in Philosophy of Science. Introduction to the foundations and cultural aspects of the sciences. Arranged and edited by Philip P. Wiener. ix + 645 pp. $5.50. Scribner's, New York. 1953.

"PHILOSOPHY in the full sense," wrote William James in one of these readings, " is only *man thinking.*" This volume includes a selection of what some philosophers, some scientists, and some others, have thought modernly about science. The thinkers represented include not only contemporary writers, whose excerpts constitute the major part of the book, but also such modern philosophers, to mention only a few, as William James, John Stuart Mill, Henri Poincaré, Darwin, and Aristotle. Philosophy of science is unfortunately often subject to unsatisfactorily superficial treatment by scientists who naively presume to have settled philosophical problems whose very existence they but dimly even recognize; it is the virtue of the editor that he has succeeded admirably in choosing authors who are qualified in their fields and who express themselves lucidly.

The contents of the book are grouped around four main subjects: mathematics and physical science, biological and psychological concepts, methods and problems of social science, and philosophical analyses and syntheses. It is itself an illuminating commentary on the thought of our times that the longest of the four sections is that dealing with social science. The selections have been deliberately chosen at various levels of specialization for the benefit of readers with differing backgrounds.

The selections consist in general of full chapters or articles or of reasonably long excerpts, so that as far as possible coherent rather than fragmentary concepts are expressed for each author quoted. Mere chapters of a man's thought are not always enough to achieve full success in this respect, and many of the quotations stop in the middle of an idea. These, however, present a lively challenge to the reader to pursue further the interrupted idea, and the important fact remains that they actually do contain many ideas. That is a great deal to say of any book, and this particular one is enthusiastically recommended for its able and agreeable presentation of the ideas man is thinking about science.

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THOMISTIC PHILOSOPHY

The Philosophy of Human Nature. George P. Klubertanz. xiii + 444 pp. $3.50. Appleton-Century-Crofts, New York. 1953.

THIS volume by an assistant professor of philosophy at Saint Louis University is a text designed primarily for Catholic students of philosophy. The title of the book is rather misleading, since the author's avowed objective is to present "a Thomistic philosophy of human nature" in which St. Thomas' thought on man is developed systematically in a modern setting. "In the entire course of this argument, practically no notice is taken of other systems" (p. 10).

The book is in two parts. The first part, or text, presents a systematic, clear analysis of classic topics of Thomistic psychology, such as, the unity of man, vegetative life and the soul, activity and power, knowledge, sensation, the intellect, the will, the sensory appetites, habits, and the origin and destiny of the human soul. The results of each chapter are summed up in definitions and demonstrated by proofs in syllogistic form. A concluding chapter (xiv) presents a systematic summary of the entire discussion of human nature, supplementing "the way of discovery" of the text with "the way of judgment." Thus, within the compass of a few pages (349–52) the student is presented with an

unequivocal doctrine of the origin of the soul and syllogistic proof of its immortality and divine origin.

The second part of the book (357–436) contains two groups of appendices comprising brief analyses of opposing philosophical systems and discussions of topics not directly part of the course. Approximately two pages are devoted to each of the following subjects: dualism (the Platonic man); idealistic monism (the spirit man); materialistic monism (the mechanical man); positivism (the unsubstantial man); sensism (the animal man); philosophical Freudianism (instinctive man); philosophical evolutionism (the evolving animal); determinism (the predetermined man). There is no discussion of historical idealism or of historical materialism (Marxism).

One of the serious limitations of the book is its almost total preoccupation with classic problems of Thomism. There is no reference to the literature of modern anthropology and culture history, although Catholic ethnologists, such as Fathers Wilhelm Schmidt and Wilhelm Koppers, are keenly aware of the direct relevance of this subject for any discussion of the philosophy of human nature.

I do think, however, that the author has rendered a valuable service to American philosophical scholarship by his clear and systematic presentation of the Thomistic point of view. With the realization that the concept of man constitutes a common ground for students of philosophy and the social sciences, will come a more fruitful approach to the study and resolution of our human problems.

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MEASURES OF MEN

Historical Metrology. A. E. Berriman. xvi + 224 pp. Illus. $3.75. Dutton, New York. 1953.

HERE we find an engineer exploring the libraries, museums, galleries, and other sites of culture, gathering information on the rich heritages of civilizations and methodically coordinating it into a basic foundation of metrology. He appreciates the true significance of each archaeological item which must fit in the picture.

First, he relates the Greek foot measure to 1/100 second of arc of a great circle of the earth, thus harmonizing the sexagesimal system. Next comes the English acre ". . . the most intriguing of ancient measures . . . virtually equal to the myriad millionth of the square of the terrestrial radius. . . ." Standards are discovered which are based upon the density of water and of gold, each of geodetic origin.

Three constants are called upon to harmonize the values of many ancient measures; $k = 1.296 = 1/10000$ of $360° \times 60' \times 60''$, π and $\sqrt{2}$ the chord-length ratio of the quadrant of a circle.

From recent precision measurements of ancient structures (the Greek Parthenon and many others), Berriman has recreated units and compared them with standards, with surprising coincidences.

More striking is the series of comparisons drawn from the English acre, which in circular dimension can be inscribed by the square Scottish acre, which, similarly expressed, can be inscribed by the Irish acre. (This Irish circular acre has a diameter near that of the earthwork circle at Stonehenge.) A square enclosing this circle will contain 10,000 square yards and is probably equal to the ancient Hindu land unit, nivartana. From the amounts of seed sown per unit of area he confirms the related units as in accord with good practice. The methods employed "make the evidence speak," the ideal of all expert investigators.

His reference to Mauton of Lyons, who in 1670 proposed a geodetic minute of arc decimally divided, makes it necessary to reappraise the idea of the metric system as being an invention of the 18th century.

Many scientists will find lore and facts pertaining to their specialty. Historians, antiquaries, appraisers and serious numerologists will find enjoyment in studying this volume containing a world-wide coverage of ancient metrology. Numerous cuts, drawings, and tables present the material in a realistic manner.

Answer to the proposed question, "Was the earth measured in remote antiquity?" seems to be "Probably."

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A FRIEND OF BOLIVAR

BOUSSINGAULT. Juicio critico del eminente agronomico del siglo XIX, Su viaje a la gran Colombia y sus relaciones con El Libertador y Manuelita Saenz. (Critique of the eminent agronomist of the 19th century, his journey to greater Colombia and his relations with the Liberator and Manuelita Saenz.) Carlos E. Chardon. 109 pp. Plate. Editora Montalvo, Ciudad Trujillo, Republica Dominicana. 1953.

THIS is a lively and entertaining introduction to a French scientist who has long been shrouded in anonymity. The apt, perhaps too brief, passages quoted from Boussingault's own writings give it flavor, and reveal an inquisitive, humorous, very personable young man and the life he led in 19th century South America.

The warm chronicle of the man is presented largely through appropriate selections from his little known *Memoirs,* published in a very rare limited edition in 1892. The Spanish text consists of four principal parts. The Prologue (pp. 7–18) treats of Boussingault's life up to the age of 20 when he left for Venezuela with a letter of introduction from his friend Humboldt to Simon Bolívar; The Journey (pp. 19–60) is an account of his travels through Venezuela, Colombia, and Ecuador with 13 brief and enjoyable sketches from the *Memoirs*; "Europa" (pp. 61–87) describes Boussingault's return to France after 10 years in South Amer-

ca, records his activities until his death in 1887, and adds a critical review of his scientific studies; "Boussingault, el Libertador y Manuelita Saenz" (pp. 89–109) completes the series with an interesting historical sidelight which reveals Boussingault's familiarity with General Bolívar and his *cara amica,* Manuelita. The anecdotes provide intimate light upon a situation which Spanish-American historians have held incompatible with the memory of the George Washington of South America, and have depreciated as showing no respect for Bolívar. Boussingault's Manuelita, however, is less restrained; he portrays her as an "extraordinary," "beautiful," "irrepressible," and "eccentric" *gran señora.*

Chardon's study is not an abridgment of the five-volume *Memoirs.* It is a series of carefully selected passages, judiciously set into the author's narrative and annotated with 58 footnotes to other references and historical notes. Chardon gives an analysis of Boussingault's 213 contributions to fields of science, based on the bibliography cited by Alfred Lacroix in *Notice historique sur Jean-Baptiste Boussingault,* Paris, 1926. The task of selecting the passages and of combining them with the commentaries is well done, with a scholar's patient attention to detail. The result justifies the author's determination to let Boussingault speak for himself. The volume merits translating into English for it provides excellent and instructive reading especially at this time when North Americans are seeking so much in common with Latin-Americans.

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NATURE AND NURTURE

Free and Unequal. The biological basis of individual liberty. Roger J. Williams. xiii + 177 pp. Illus. $3.50. University of Texas Press, Austin. 1953.

THIS book is a vigorous presentation of the fact of genetic variability in man and of the serious consequences for human relationships that follow a neglect of it. We can be equally free, the author points out, without being uniform. There is a deep conflict between this idea and the one tacitly assumed by many educators, sociologists, and medical men—that all people are born fundamentally alike and that the differences they later display are the result of the environment in which they are reared. This ancient controversy between the advocates of "nature" and "nurture" is still very real, says the author, and underlies the chief ideological conflict in the world today. If there were no inborn distinctions between human beings, the importance of the individual would vanish and freedom would lose its significance.

The author presents evidence for the existence of a "distinctive and complex pattern of inborn mental capacities" in every individual which is unique for the individual. It is not *man* and his common traits which finally are important, but *men* and the great diversities they display.

Education, he believes, is too often an "assembly line" process which endeavors to *mold* people into a social pattern rather than *fit* them into it by developing their particular capacities. Physicians are apt to generalize about "the patient," whereas each patient is an individual problem. To attempt to treat all workmen alike is also fruitless for they are different, not only in productivity but in many other ways. The radical differences in aesthetic tastes and religious beliefs have a biological basis and should not be the cause of violent disagreements. Harmony is much more likely to come from a friendly recognition of differences than from attempts to make everyone think and feel alike. Disagreement is natural and healthy. We should, of course, oppose discrimination on such grounds as race and religion, but let us not forget that there are real differences among people and that these must be recognized if we are to live happily together.

With the author's viewpoint one must be sympathetic. It is well to have the case for "nature" so strongly stated. The uniformity and regimentation of totalitarianism we abhor; but too great emphasis on inherited differences can lead to racism and Hitler. We should remember that what is inherited is a series of *capacities,* not of traits; a repertoire and not a fixed program. How these capacities are realized depends on training. Man is enormously educable. Our attitudes, motives, and tastes are determined largely by the kind of environmen in which we live. To gain the highest satisfactions of life and to establish the Good Society we must recognize not only human differences but the importance of guiding these differences into the best channels.

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LETTERS

MATHEMATICS AND THE EDUCATIONAL OCTOPUS

THANK you for presenting at last a discussion of educational policies in which an effort at least has been made to approach the subject with a proper thoughtfulness. It is too bad that Cairns was not able to sustain his desire to illuminate rather than to inflame throughout his paper entitled "Mathematics and the Educational Octopus" in your April number. Perhaps the selection of a pejorative term such as "octopus" in the title itself indicated that it would not be possible for the author to avoid acrimony.

The most regrettable feature of this attitude is that he has been betrayed, perhaps unwittingly, into the use of debater's tactics in brushing off the weightiest piece of opposing evidence as if it were of no importance. Now perhaps it really is not important. But we shall never be able to judge from the cavalier treatment accorded it by Cairns.

His dismissal is so casual that little effort is involved in quoting it in full:

It is interesting also to glance briefly at the so-called Eight-Year Study, used by many educationists as a justification for experimentation. Although it purported to be a controlled statistical study, Vol. I of the report, *The Story of the Eight-Year Study,* states: "Everyone invited to serve on the Commission was known to be concerned with the revision of the work of the secondary school and eager to find some way to remove the obstacle of rigid college prescription." Little wonder that this report as a whole is a gold mine for those seeking samples of statistical fallacies!

Boiled down, this paragraph tells us what?

(1) The people who issued the report admitted their antecedent bias in favor of the conclusions reached;

(2) A mathematician, Cairns, has alleged that there are many statistical fallacies in the report.

Do these items establish that the conclusions of the report were incorrect? Who can say, when Cairns' brief glance is so brief that he does not even tell what the conclusions were except by indicating that they were "justification for experimentation" of some unspecified nature?

If, Sirs, you are concerned with dealing a body blow at Cairns' alleged octopus, then I can think of nothing which would carry more weight than a full and convincing analysis of the reasons for discrediting the major conclusions of the *Eight-Year Study.*

If you wish merely to amuse us with glib ironies, then I should think it would be fun to turn a witty psychiatrist loose on the question of why it is that both sides of what Cairns evidently wishes to view as a "battle" have a tendency to suspect the perfidious opposition of concocting foul plots for the overthrow of all that is good, beautiful, and true—including, of course, themselves.

If you are, by any chance, concerned with serious inquiry into the areas of our ignorance about the learning process, then I should like to suggest a few questions to chew on:

(1) Cairns asks, "Why is it that they should not be obliged to master such useful tidbits as the multiplication tables?" I should like to ask why is it that the conservatives in education never seem to worry about inculcating the addition tables by rote, even though they contain just as many combinations of numbers as the multiplication tables? This is a serious question, not a sarcastic one; for I have noted that it is apparently true that addition is learned more readily than multiplication; and I am curious as to why this should be so, if it is so, as I suspect an understanding of what is going on might be very helpful in teaching arithmetic.

(2) Cairns states that "In English classes, the parts of speech are either not taught at all or are supposed to be picked up in some incidental manner—perhaps after ignorance of them has hampered students in foreign language courses." May we infer from this statement that ignorance of "the parts of speech" hampers students *only* in foreign language courses, and *not* in the use of their native tongue, save perhaps for an awkward, but possibly not grave, embarrassment when one of the semi-literate devises some such locution as "between you and I"? If so, does this condition indicate a fundamental distinction between English and the foreign languages conventionally studied in college? Or does it perhaps indicate that the conventional methods of instruction in the colleges are a decade out of date? These are serious, not sarcastic, inquiries.

(3) Cairns exclaims, "Why we should shun the development of memory as a tool is a mystery difficult to fathom!" Avoiding the invidious assumption that the concept of "development of memory" may be suggested by a half-remembered echo of the long-abandoned belief in "formal discipline," let us assume that we have here an expression of a genuine interest in learning how to make our memories more effective. Let us, then, consider for a moment the implications of a rather striking little demonstration that is popular in elementary courses in psychology. The instructor reads in a monotone a group of syllables such as:

blee — wang — flate — jy — muke — drit — yoll — zux — lape — strell.

The class is then asked to write down what they can remember. No one remembers all of it. Most recall one or two.

Again the instructor reads a group of syllables in a monotone:

tree — grass — wind — leaf — snow — hail — rain — sun — fire — cloud.

Again the class is asked to write. A gifted few may remember all or nearly all the syllables. Most get several of them. Thus the importance of familiarity—the results of "tedious drill" may be estimated; actually

of course, the "drill" that led to this result may not have been "tedious," because it may have been the by-product of that dubious entity, "life experience."

Again the instructor reads in a monotone: two — big — planes — came — down — fast — to — the — new — field.

Everyone busily writes; and this time nearly everyone is perfect. What has happened? It is easy enough to say that "meaning" has been demonstrated to be a more powerful aid to memory than years of drill. But *why* is it so? And *how* are we to go about sponsoring experiences which will use this power, if, as Cairns suggests, we are to shun the "grocery-store concept of mathematics"?

A serious consideration of such questions might, I should hope, lead critics like Cairns, who are trying to be objective, to see that if we are to make any real headway in learning how people can learn, not less, but more—an end devoutly wished by all of us—we ought not to hamstring our inquiry by starting out with the assumption that we can identify by inspection certain "essential elements of our intellectual heritage," the values of which must be accepted as an axiomatic without further questioning.

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NIGHT OR DAY

REGARDING the interesting article by Nathaniel Kleitman and Hortense Kleitman entitled *The Sleep-Wakefulness Pattern in the Arctic* which appeared in THE SCIENTIFIC MONTHLY, 6, 349 (1953), there is a general misconception about arctic winters which the Kleitmans apparently have accepted and which, I feel, has certain significance in this study. The article states that at Tromsø, Norway (69° 39′ N) "daylight . . . decreases (after the autumnal equinox) down to nothing by November 21 when a period of complete darkness begins" (p. 350, 2nd par., 6th line).

This popular notion that all people living north of the Arctic Circle spend a prolonged period each winter in total darkness harks back to our primary school geography lessons and is erroneous. Although I have no figures on the light during the winter at Tromsø, I have some for Point Barrow on the northern tip of Alaska and more than 100 miles north of the latitude of Tromsø. Although the sun does not appear above the horizon for about two months in the winter, there are several hours of good daylight each day.

The following are representative figures copied from a daily light table issued by the Navy's Aerological Project at Pt. Barrow for December 1952. The exact criteria used for such things as "usable light" (described in the table as light sufficient to make "objects definable at approximately 100 yards") and "beginning" and "end of twilight" are a little vague. The table was drawn up as an aid in planning outside construction and maintenance work in the Pt. Barrow area. During the period of "usable light," no artificial illumination was required for outside activities.

Date	Begin twi-light	Begin usable light	End usable light	End twi-light	Hr–Min usable light
1	0659	0913	1520	1742	6–07
11	0707	0932	1510	1732	5–38
21*	0716	0940	1509	1734	5–29
31	0716	0940	1520	1744	5–40

* Shortest day of year.

Even during the middle of December the light at noon in Pt. Barrow is quite similar to that at noon on a dull dreary day in, for example, New York City at the same season. It should be kept in mind that a snow-covered landscape such as is found in northern Alaska and, I presume, Tromsø, brightens the scene considerably.

In Figure 1B in the Kleitmans' article (the graph showing the getting-up time at Tromsø) the line reaches a winter maximum at about 7:30 A.M. At this hour in northern Alaska during December, there is twilight. It is, to be sure, a very dim twilight which progresses very slowly. But is it not true that most of us even in the latitude of New England are accustomed to arising during or even before twilight in the winter? In Canada and the British Isles this is even more the case.

It is not too surprising that the winter going-to-bed and getting-up times of the Tromsø inhabitants are practically the same as those of inhabitants of similar-sized cities in the northern temperate zone (taking into account that the lights in Tromsø go out at 9:00 P.M.), since the situations are not very different. In both places, the people go to bed in the winter long after dark and get up either before or during morning twilight.

If Tromsø did, indeed, have a prolonged period of total darkness, then any differences or similarities between the habits of Tromsø inhabitants and the inhabitants of similar-sized cities in the northern part of the temperate zone might take on real significance.

In review, the Kleitmans, in studying the sleep-wakefulness pattern of the people of Tromsø, have apparently been misled into believing that those people live in "complete darkness" for about two months each winter. Although the amounts of daylight in summer and in winter differ to a huge degree at 69½° of latitude, which in itself makes for an interesting study, the authors misstate the case when they say that "continuous darkness prevails in the winter" (p. 356, last par., 3rd line).

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ON ANIMISTIC THINKING

THE report of Wayne Dennis' survey of animistic thinking among the educated [THE SCIENTIFIC MONTHLY, **76**, (4), 247 (1953)], shows with startling clarity how little we know what others think and how prone we are to assume, without justification, that our

very elementary concepts are accepted by nearly all others with equivalent education or experience.

But I am not convinced that such a high percentage of educated adults, if asked fairly, could fail to answer correctly such elementary questions. A study of the instructions used and some experiments with friends suggest that their very sophistication betrays the subjects.

The connotation from these instructions is that some of the questions are not at all simple and that some are hard. The subjects strive to find an answer that is not obvious, for on the surface, they are all simple and none is hard, and so striving, they fall into an unintentional trap.

I believe that if in substance the instructions were, "I am going to ask some simple questions. I want a yes or no answer to a literal question," the results would be different. The significant word is "literal." If these tests do not confirm Mr. Dennis' first findings, his work will still be valuable, for it will show how easy it is to invalidate a test by apparently unimportant details connected with its administration.

I think your readers would like to learn if further tests confirm the low level of educational content among our educated indicated by the first survey.

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Dr. Baëza's suggestion that it be made clear, that what is desired in response to questions concerning the animation of objects is a literal answer, is an excellent one. I hope to be able to make use of this suggestion in the near future. However, as I indicated in my original paper, the majority of the answers I have received indicate that the questions have been accepted literally.

So far as the responses of Dr Baëza's friends are concerned, it should be noted that research scientists and their friends should not be confused with the persons whom I questioned, who were public school teachers and average college students. On the whole there exist between these classes of persons great differences in ability, as well as differences in scientific training.

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THE GEOMETRIC WISDOM OF SOLOMON

Gridgeman in his *Circumetrics*, The Scientific Monthly, **77**, 31 (1953), has underestimated Solomon. Since this wise king probably knew that π was transcendental, he was smart enough to round off the dimensions in II Chronicles IV: 2 to the nearest significant figures, even as statisticians do now. By accepting the inerrancy of the Scriptures and using the correct value of π, it follows that the circumference of Solomon's molten sea in cubits was between 29.85 and 30.50, and the diameter between 9.50 and 9.71, or 30 and 10, respectively, to the nearest even cubit.

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ERRATUM

In the article "Circumetrics" by N. T. Gridgeman that appeared on page 31, The Scientific Monthly, July, 1953, there is an error in the equation on the fourth line, second column, of page 32. The equation should be: "$\arctan x = x - x^3/3 + x^5/5 - x^7/7 + \cdots$" and not: "$\pi/4 = 1/(2 + 3^2/(2 + 5^2/(2 + 7^2/(2 + 9^2 \cdots$" as printed.

N. T. Gridgeman

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A NEW AAAS EMBLEM

The Board of Directors of the Association has approved the design reproduced at the left as a symbol of identification with the AAAS. In the future, this design will appear on the symposium volumes and will be used for other appropriate purposes.

The Association will soon make available to its *members* lapel buttons and pins. (Keys will also be provided if a sufficient number of orders for them is received.) The size will be identical with the illustration. The scalloped border and the lettering will be in rolled gold, the background in blue enamel, and the torch in red enamel. The key, if provided, will have the basic design superimposed on a black enamel background with a second rolled gold border.

Information on prices and how to order insignia will be sent to all members and will appear in our journals in the fall.